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1. Sainz, A.: To be published. 2. Noce, R. H., Williams, D.B., and Rapaport, W.: In press. 3. Kline, N.S.: Ann. New York Acad. Sc. *59*:107 (April 30) 1954. 4. Catlett, J.M., and Gunn, C.G.: To be published. S/2053M

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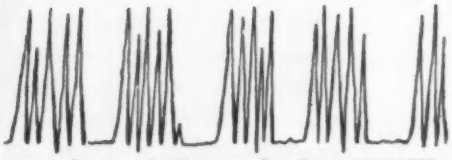

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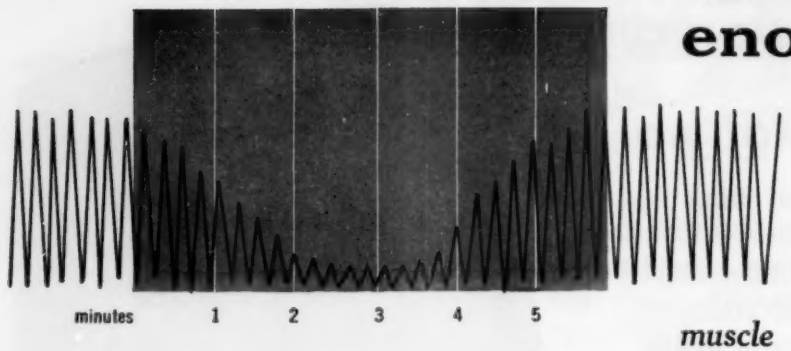


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X

# DO OUR MEDICAL COLLEAGUES KNOW WHAT TO EXPECT FROM PSYCHOTHERAPY?[1]

KARL M. BOWMAN, M.D., AND MILTON ROSE, M.D.[2]
SAN FRANCISCO, CALIF.

A title in the form of a leading question is commonly used to provoke thought and to dispel complacency. We frankly confess to this purpose, our intention being to present a factual report and personal commentary on psychotherapy—a form of healing which in our times has changed the face of medicine. This change has taken the shape of a spectacular boom in psychiatry—a phenomenon which is primarily a reflection of the public acceptance of the effectiveness of psychotherapy. Our shock and other physiological therapies and our progressively more efficient and humanitarian hospital practices have spread our influence, to be sure, but no one who observes even casually the public temper would deny that everyday usage of the term "psychiatry" is almost synonymous with "psychotherapy." We ourselves as specialists are inclined to soft pedal our physiological therapies, and even to become apologetic about them. Indeed, most of us feel that psychiatry's main claim to distinction as a specialty rests on a system of psychotherapy based on a sound theoretical framework customarily called psychodynamics.

To get back to our title, which raised the question as to whether our fellow physicians are clear about psychotherapy, this audience has doubtless already surmised our answer. It is, in general, "no." That we are frequently misunderstood and even maligned by our medical colleagues, sometimes fairly and sometimes unfairly, is a truism which the experience of the members of this audience will surely corroborate. Our own experience—we have not made a formal poll of medical opinion—indicates that a great many physicians are muddled in important respects about the methods, goals, and effectiveness of psychotherapy. We will go further and suggest that this muddle is in considerable part a reflection of our own muddled state. Evidence for this is plentiful in the voluminous professional literature which is pouring from our busy brains.

That all of us in medicine, psychiatrist and nonpsychiatrist alike, should be confused in our views and expectations of psychotherapy appears to us to be inevitable, because of the nature of the times and of ourselves as physicians (with respect to our training) and the nature of the human mind which is the object of our study and practice. Happily, this situation is not without remedy. There appears to be an inherent self-correctiveness in major movements, whether they be scientific, artistic, or social—a sort of natural "feed-back" propensity. The signs of the times indicate that this tendency toward self-correctiveness has already begun to appear in psychiatry. This paper is itself a reflection of such a trend, and other examples are not hard to find.

In the first place, there is an increasing interest in quantification, always a sign of a developing critical, or, you might say, scientific attitude. Science, broadly speaking, requires the invention, testing, and confirmation or rejection of hypotheses to fit the data, and the possibility of making accurate predictions on the basis of these hypotheses. Psychiatric efforts in this direction include studies, many of them on a major scale, now in the early stages, designed to test the effectiveness of various psychotherapeutic methods. In addition to these studies, there are increasing references in psychiatric literature to epidemiology, a quantitative medical science which is concerned with disease in the mass and the factors which play upon the mass to produce the disease (ecology). The conference held in 1949 by the Milbank Fund on "The Epidemiology of Mental Disease" is a landmark of great significance in this development, and there are many other instances of a growing interest in the

[1] Read in the Section on Private Practice at the 110th annual meeting of the American Psychiatric Association St. Louis, Mo., May 3-7, 1954.

[2] The Langley Porter Clinic, Department of Mental Hygiene, State of California, and the University of California School of Medicine: Psychiatry, San Francisco, California.

use of epidemiological methods to help us elucidate our perplexing scientific problems.

Most encouraging of all, attempts at critical evaluation in the form of reflective essays and editorial comments have come from within our own ranks—a most important sign that through the operation of our "feedback" mechanism, we are acquiring a new and more appropriate perspective as a medical discipline. Critical comments on what have been called our expansionist or global tendency—a tendency which suggests a belief that we have already developed the technical wherewithal to make definitive contributions toward anything from preventing sin to preserving international peace—have come from our own country and from Great Britain, and undoubtedly similar opinions have been expressed in other countries.

Out of these and similar attempts at critical evaluation and self-criticism we can hope for a continuing clarification of those problems of psychiatric theory and practice which contribute significantly to the present state of confusion among doctors of all kinds, psychiatric specialists included, about the true status of psychotherapy.

## MEDICAL EDUCATION

By far the largest part of the prevalent dissatisfaction with and misunderstanding of psychiatry is concerned with psychotherapy. The specific physiological therapies which we use in such disorders as schizophrenia, involutional depressions, cerebrospinal syphilis, etc., produce only a minimum amount of friction with our colleagues in other specialties, even though, as in medicine generally, these therapies may not be invariably successful. We are aware that nonpsychiatrists are wont to explain their reservations about psychotherapy by charging (1) that many psychodynamic concepts and formulations are of an esoteric nature; (2) that psychotherapists too often remain aloof and isolated from other types of physicians, and (3) that we take too long and charge too much to accomplish our purpose. There is, to be sure, a certain validity to these explanations, but we have become convinced that the factors actually determining medical attitudes toward psychotherapy and psychotherapists are more fundamental than the usual explanations would indicate.

To understand these fundamental factors, it will be useful to glance briefly at the present status of medical education. There are clearly three dimensions to the influence brought to bear upon medical students. The power and prestige of each of the dimensions is reflected in the number of hours devoted to them in the medical curriculum. These dimensions are: (1) the physico-chemical-biological approach, (2) the psychological approach, and (3) the social approach to normal and abnormal functioning. The first of these, the physico-chemical-biological approach, is the most powerful, having long been the distinguishing characteristic of scientific medicine.

Professor Dana Atchley(3) in his recent *Saturday Review* article, "The Healer and the Scientist," pointed out that the great tendency in American medicine has been to bring experimental thinking and methods (physico-chemical-biological) into the clinical years. A reflection of this is the rapid growth of full-time medicine, which actually means that more clinical teachers than ever are spending all of their professional time in the medical school on a program which consists of teaching plus investigation of fundamental medicine problems, pure or applied.

The second dimension, fortunately for millions of sick and suffering human beings, has grown considerably, especially since the end of the recent war. It consists in the study of psychological factors in disease. Instruction consists in (1) the presentation of a system of psychodynamics—sometimes several systems, depending upon the diversity of points of view among members of the psychiatric department, and (2) the study of the clinical characteristics of the major categories of psychiatric disorder. The psychological dimension of medical education is relatively small, as judged by the number of hours devoted to it, in comparison with the physico-chemical-biological, and for reasons too obvious to require discussion, will probably always remain so.

It is not surprising that the medical graduate of today is a better all around practical scientist than was his counterpart in past

years. His training has provided him with the intellectual and technical tools to deal with human disease in the precise concepts and language of chemistry, physics and biology; his awareness of the power, often crucial, of emotional forces in illness is more acute and formal than before; he is more inclined to recognize his responsibilities to society at large and to adapt his activities to encompass such things as group practice, pre-pay plans, and other economic developments evolved by circumstances in our present world.

Reinforcing or perhaps reinforced by this medical training is another influence of great importance in the molding of the modern physician. This is the fact that he is, to begin with, a human being who lives in what Stainbrook has aptly called "the age of the self-conscious man," that is, the present age of science which includes in its domain the formal study of man himself, not only his physical being, but also his personality.

## MEDICAL BELIEFS ABOUT PSYCHOTHERAPY

This combination—the scientific temper of the times, with its assumption that all natural phenomena (including mind) are susceptible of scientific explanation, plus a rigorous indoctrination in the application of scientific method to the study of the human organism, plus an awareness of the tremendous power and importance of emotions and ideas in human life—makes it inevitable that the doctor of today takes it for granted that a science of mind or personality that can fulfill the theoretical and empirical requirements of science is not only possible, but indeed already exists.

It is also inevitable that when these doctors are called upon in practice to make use of and to evaluate this science of mind, as it is presented by those medical men who are its specialists, they judge it according to those scientific criteria that apply to the explanation and treatment of the vast number of medical surgical disorders. Physicians quite naturally make use of the logic of science and their own experience with scientific method as a clinical approach to the problems of medical practice. This expectation that the clinical data of psychiatry can be treated in a manner comparable to that used in handling the data of physics, chemistry, biology, etc., is fostered by us psychiatrists. In our eagerness to live up to our own requirements as scientifically trained physicians and to the demands of the world for a scientific understanding of the mysteries of the human mind, with its propensity to sicken and its equal resistance to destruction, we have been tempted to believe that our clinical knowledge of the nature and functioning of the mind was keeping pace with developments in the physics and chemistry of the physical organism, that our knowledge of psychodynamics was in fact rapidly cumulative.

It is no wonder that the physicians of other specialties tend to believe that: (1) we know or are on the verge of knowing the etiology of mental disorders (those often referred to as the functional neuroses and psychoses); (2) that many cases are "curable"—in the sense that diphtheria is curable, for example; (3) that psychiatry has developed a fund of well-organized subject matter which if mastered by the psychotherapist and used by him in a highly technical manner will result in certain predictable changes in at least some of his patients, and that he will be able to say with some precision how these changes were brought about, or if not, why not; (4) that certain of the psychotherapeutic methods are more thorough, "deeper," more effective than others.

Now, much as the authors, as psychiatrists, would like to agree with these assumptions and many related ones which might be cited if time allowed, the facts do not permit us to do so. We do have the firmest kind of conviction that psychotherapy carried on under the aegis of medicine provides for medicine and mankind a type of help which is of the first order of importance. Psychotherapy is a fact of nature which was undoubtedly recognized by Adam and Eve. Alone in the Garden, they must have been their own therapists. But while admitting the importance and efficacy of psychotherapy in its many forms, we cannot emphasize too strongly the necessity for being as rigorous as possible when we make scientific judgments about the methods, goals, and effectiveness of psychotherapy. It is easy for us to be carried away by our natural desire

(born of our medical training and the expectations of the modern world) to feel that our work is based upon a growing body of precise knowledge, and even to claim, perhaps, that we are having some difficulty in keeping abreast of its rapid advances.

Thus psychiatrists have spoken glibly of "discoveries" and "new insights" in the course of psychodynamic investigations and subsequent formulations. Truth to tell, these "discoveries" if accurate at all have in most cases been restatements in modern idiom of truths about the human mind which have been known to the wise men throughout the ages, including, of course, many psychiatrists in our own age. How disconcerting, and yet inspiring, to browse in the literary, philosophical, and religious classics and to find equivalents for most of our terminology and the ways in which we use it to "understand" and "explain" man.

## Some Sources of Confusion

### PSYCHODYNAMICS

We have already stated our belief that the muddle which exists about psychotherapy among nonpsychiatrists must perforce be related to our own pressing intellectual and emotional problems as psychiatrists. Even though we are uncertain and unagreed, we give an impression of being certain about our basic tenets—a condition which must surely be born of an impelling desire to be on the same scientific map with coordinate branches of medical therapy.

Our most pressing problems, those bearing on the scientific nature of psychotherapy, are also the most important sources of confusion to the nonpsychiatrist. Among the most crucial of these, and certainly the most interesting, are those falling under the broad headings of psychodynamics, and long *vs.* short therapy, and, related to the latter, the question of goals of psychotherapy (cure *vs.* meliorism).

We urge everyone to read *The Psychiatrist, His Training and Development*(1), to dispel any doubts about the urgency of the problems of psychodynamics. The volume, prepared under the editorship of several of our distinguished colleagues, illustrates the present status of American thinking about psychiatry generally, and most especially about psychotherapy.

This is a valiant attempt to formulate a scientific basis—a psychodynamics—for our psychotherapy, but it appears to us to be only partially successful. It is not surprising that the wording of the report on psychodynamics is at times almost tortured, so great are the differences among the essentially irreconcilable opinions it must compose. A clear indication of the highly subjective nature of the opinions of the contributors to this discussion, contributors who represent some of our most talented and serious psychiatric thinkers, is the statement which appears on page 19:

> In formulating psychodynamics, individuals tend to select their postulates with strong feelings of conviction, in accordance with private feelings or group allegiances rather than public knowledge.

We submit—if psychiatric scholars and practitioners of distinguished experience find it necessary to describe their methods of arriving at psychodynamic formulations as being mainly subjective, we ought not to be surprised at the bewilderment of our science-minded colleagues in the more concrete fields of medicine.

### LONG-TERM VS. SHORT-TERM PSYCHOTHERAPY

The controversy over long *vs.* short therapy is also baffling to the nonpsychiatrist, and he is only temporarily convinced by such forced and artificial analogies as: long psychotherapy is "deeper," "gets at causes," is a kind of "surgery" of mental disorder; short psychotherapy is more "superficial," deals with symptoms rather than with causes, is essentially a palliative treatment of what, if time, expense, and suitably trained psychiatric manpower were no question, would call for long psychotherapy.

Psychiatrists, too, are often unconvinced by such debates. In the first place, a true or false approach to the question is a futile waste of time. In the studies which have been made thus far, comparing the effectiveness of different psychotherapeutic methods, no differences have been uncovered.

This is in part due to the tremendous difficulty which everyone encounters in setting up objective criteria of improvement. Sharp

therapeutic end-points in our field are few and far between, with the exception of the reversal in severe mood disturbance and marked disturbance in social behavior as in schizophrenia, certain psychopathic states, etc.

The percentage of improvement in the treatment of the psychoneuroses in 11 different studies published since 1934 shows a range from 55 to 87% of the patients treated (with an average of 67%) improved. Results in psychotherapy with the schizophrenias are very similar—a range of from 25% to 50%, regardless of the methods used.

Studies of this kind are and will continue to be critically influential. In our judgment, selection of cases for study and criteria for improvement can be made much more readily than has been thought possible, provided we focus on rigorous description of symptomatology and signs and the more demonstrable evidences of change or improvement. Relatively vague concepts such as assumed similarity of psychodynamics, and criteria such as "better adjusted" and "happier," have meaning but do not lend themselves easily to quantitative evaluation of treatment methods.

In a provocative discussion of the similarity of results in psychotherapy by different methods and schools of thought, Appel, Lhamon, Meyers, and Harvey concluded that psychotherapy was a crucial factor in improved cases, and that the common denominator of improvement has nothing to do with the psychodynamic formulations and methods of the specific psychotherapies studied, a point of view with which we readily concur. Why the 37% failed to improve is next to impossible to say with accuracy.

## PSYCHOTHERAPEUTIC GOALS

We have further confused our medical colleagues by our discussions of therapeutic possibilities in psychotherapy, where we have sometimes suggested an analogy between psychiatric disease and the other disease categories with respect to cure *vs.* meliorism. Frequent use of the term psychodynamics—now virtually a shibboleth—strongly suggests that we have a knowledge of cause in our "functional" disorders. Being human, we are inclined to be overoptimistic, and being, moreover, physicians in an era of science (or hyperscience, as someone in a facetious mood recently called it), we are inclined frequently to talk in scientific terms where our knowledge does not permit such satisfying simplification.

Thus, the terms "cause" and "cure" (the term "motivation," though having a different meaning from "cause," is often used synonymously with it, with confusing results) appear often in discussions about psychotherapy. When we talk about "deep" psychotherapy (usually equated with long psychotherapy), we imply, if we do not explicitly state, that we are dealing with causes.

Physicians assess favorable therapeutic results in terms of complete cure or improvement, the latter sometimes referred to as melioration. Complete cure with disappearance of signs and symptoms occurs when the causative agent is neutralized (rendered innocuous) and removed. Infectious diseases and certain congenital and acquired structural defects are examples of such "curable" conditions. The most important of the "killer" diseases—cardio-vascular, endocrine, and cancer—usually lend themselves to meliorism—improvement only—but no one questions the value of improvement if complete cure, that is eradication of the cause, is impossible.

But in psychiatry we are dealing with maladaptations of that very hard to define concept—personality—not with a very readily definable infectious disease. We have no highly specific process or technique to offer that can do something about cause or course. What we have to offer is ourselves—ourselves as "professional friends." As such we offer our services in a comparatively unhurried manner in an atmosphere which encourages our patients to express their thoughts and feelings vastly more freely than in any other interpersonal situation in existence. Success, as in all fruitful friendships, is derived primarily from the patient's susceptibility to the warmth and wisdom, gained both from general life experience and from professional experience with medical psychology, of the therapist, rather than from any specific system of interpretations of psychological material which the therapist may employ. Our psychotherapy is to be compared with education rather than with spe-

cific medical treatment, and of education there is no minimum and no limit. The decision as to how far and how long is essentially arbitrary and is determined in a highly individual way by patient and therapist together in each case. We have no set curriculum, no prescribed hours and much as we would like to be in a position to grant a symbol of successful completion, we have no degrees to offer.

Most of the confusion among psychiatrists and nonpsychiatrists alike comes from our tendency to believe that psychotherapy is more exact and scientific than it is or can be. The solution to many of our problems in relation to public and professional understanding of our specialty lies in our own hands. We ought not to condemn ourselves for failing to fulfill all the requirements of a scientific discipline, but should take pride in our ability to fulfill one of the most important needs of mankind and to fulfill it in accordance with high standards of sincerity and devotion to the best welfare of our fellows. Psychotherapy as it is in fact is vitally important and necessary to balance the impersonal, assembly-line medicine of today. Men always have and always will be struggling with problems of feelings and ideas, and as always their greatest aid and support will come from their fellows—in this day and age, from medically-trained "professional friends" as well as from those who are closest to them.

## Summary and Conclusions

1. That many nonpsychiatric physicians are confused about the methods, goals, and effectiveness of psychotherapy is in large part a reflection of the muddled state of psychiatrists themselves about the nature and status of their specialty.

2. The most important sources of misunderstanding about what psychotherapy is and is not are those problems bearing on the scientific nature of psychotherapy: psychodynamics, techniques of psychotherapy and their effectiveness, and goals of psychotherapy.

3. The training of the modern physician leads him to believe that a "science of mind" exists which in practice can be understood and judged in the same way as can the other branches of medicine.

4. This situation—the scientific beliefs and expectations of the physician plus the difficulty of establishing psychotherapy on traditional scientific grounds—results in confusion among our medical colleagues and even among ourselves.

5. A step in the right direction toward eliminating this condition will be for us as psychiatrists to be as rigorous as possible in making scientific judgments about the methods and goals of our specialty.

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## DISCUSSION

Norman Q. Brill, M. D., Los Angeles, Calif. —It is true that psychiatry is most often equated with psychotherapy and that we are less often inclined to boast of cures that result from physiological therapies than of those which result from psychotherapy. As Drs. Bowman and Rose have said, we feel that psychiatry's main claim to distinction rests on our psychodynamic concepts of personality development and emotional illnesses and the systems of psychotherapy that have developed out of these concepts.

It is interesting to speculate on why this is so: Are the physiological treatments less highly valued just because they are easier to administer, or because it has never been adequately demonstrated that they are truly physiological; *e.g.*, we call electroconvulsive treatment *physiological*, but can we really justify such a contention? Certainly, physiological changes can be observed in patients following E.C.T., but there are also psychological changes and at our present level of knowledge, it is just as probable that the physiological changes are concomitants of psychological change as vice versa.

Furthermore, most men using physiological therapies look upon them as symptomatic treatment, rather than treatment directed toward removing the cause of an emotional illness. Often they expect recurrences, especially if the patient has not also had psychotherapy. In contrast, those who have had extensive experience with psychotherapy have a conviction about the emotional basis of neuroses and the functional psychoses. Familiarity with the patient's basic conflicts and patterns of response makes it more feasible to predict how the patient will react. When one has seen a homosexual cured with psychoanalysis, and had an opportunity to observe the chronological development of the perversion as an attempted solution to conflicting impulses, one feels much closer to the truth than when using so-called physiological treatments. It is this that may make it seem more worth while.

This does not mean that physiological treatments are unimportant or that they should not be used. As long as we have nothing better to offer in a given case, and if they hold some promise of being helpful, we are obliged to use them, to improve them, and to continue to try to learn why they work.

I wonder if it isn't that the public, too, senses the basic truth of psychodynamic concepts and that this accounts for their demand for psychotherapy—not the unwarranted claims which have been made in its behalf.

One reason our fellow physicians are not clear about psychotherapy is that, for the most part, they haven't been taught enough about it. The social sciences and psychology have not been stressed in premedical curricula to the same extent that the biological and physical sciences have been stressed and as Drs. Bowman and Rose pointed out, psychological medicine has never had the emphasis that anatomy, physiology, pathology, bacteriology, and pharmacology and their clinical application have had in the medical school curriculum and later in the usual internship. How can our fellow physicians know what to expect? I am not inclined to attribute their confusions to our own confusion. Those medical colleagues who have taken the trouble to learn something about psychiatry and particularly the indications and goals and techniques of psychotherapy, instead of being defensively critical, have been our best supporters.

We don't pretend to have all the answers. To be sure, there are some psychiatrists who are misinformed or unsound, who can be pointed to to justify criticisms which are directed at all of us. We must distinguish between confusion and limited knowledge.

I would certainly agree with Drs. Bowman and Rose that we must maintain objectivity about our methods and goals and results of psychotherapy, and that there is need for humility and modesty with our limited knowledge.

I would emphasize the need to define terms, which are basic and frequently used, like "mental health" and "reality." Do we mean by mental health, how a person feels and functions by his own standards, or do we refer to his relations with society by society's standards? Does mental health imply an ability to accept reality when an effort to change it may involve the possibility of death? Does it imply the ability to get along in any culture without faltering, or does it entail the willingness to fight when it might be very unhealthy to do so? The time may come when psychiatrists may have to answer such questions for themselves as well as their patients, and the chances are that there will be differing opinions on what mental health is.

We have a tendency to equate psychopathology and disability. Yet a person may have a great deal of psychopathology but be only slightly disabled. Furthermore, the degree of disability in a given individual varies greatly from time to time in response to external changes and changing motivations.

It is true that we have no proof that we have, to any extent, decreased the incidence of mental illness despite all the psychotherapy that is being practiced and all our efforts at prevention. While all psychiatrists can point to many patients who have been helped by psychotherapy—in the absence of any truly reliable baseline of over-all incidence of emotional disorders, we have no way of knowing the over-all effect. We do not like to consider the possibility that, despite all of our efforts directed toward helping individuals and toward developing community programs, the incidence of emotional disorders may be increasing. While clear-cut neuroses are less frequent, perhaps because their meaning has been exposed, there is a strong suggestion that psychosomatic disorders are increasing. This in no way implies that we should abandon our treatment of individuals in private offices or clinics, but it does mean that we cannot continue to bank on these efforts as the ultimate solution to the problem and enjoy the feeling of making real progress, until we can prove, in ways that are more convincing than at present, that we are making a significant contribution to the over-all health of the nation.

We have no monopoly on understanding people and their motivations, and, as physicians, have been outclassed in this area by poets and writers and philosophers. Present-day psychiatry is now attempting to bring all such knowledge together into a usable composite out of which a sound system of prevention and treatment can be developed and then integrated with the rest of medical knowledge and practice.

It is true that different patients react differently to the same psychiatrist; that some patients who do not do well with one psychiatrist, will improve with

another; that the more competent psychiatrist, at times, will fail when another, less experienced, will succeed. It also happens that, at times, a patient's feeling that he has improved is not shared by the referring physician or the patient's family. We are learning much more about such things and often are able to explain them to each other. The fact that families and referring physicians often do not understand is to be expected. It is difficult for them to conceive of someone wanting to be sick or to be taken care of; they do not know the details of the patient's inner turmoil that only we are privileged to see.

Dr. Bowman has pointed out the emotional investments that our psychiatric colleagues tend to have in their particular theories of the psychodynamics and techniques of treatment of emotional disorders. That this not infrequently leads to personal animosities between members of different schools of thought is a source of amusement to our medical colleagues. This state of affairs leaves us open to the accusations that we are adherents of philosophies rather than scientific truths.

Some may think (and feel) that hostility is the basis of a patient's difficulty and that the hostility is the understandable outcome of not having had enough love in early childhood. Others are more impressed with the role of sexual repressions and trauma in the etiology of personality and emotional disorders and are convinced that hostility often stems from regressive phenomena defensive in nature. Some believe in mobilizing repressed hostility, others believe it should not only be mobilized but analyzed. Some believe it is the therapist's job to be the good mother or father and to satisfy patients' dependent needs; others feel that if lasting improvement is to result, the reason behind the persistence of such needs must be understood rather than catered to. Some encourage their patients to act out their unacceptable impulses instead of suppressing them. Others, who perhaps identify less with their patients, try to help them understand the origin of such impulses. It is just such differences as these that make it difficult for psychiatrists to agree on what improvement is. There is no doubt that we very much need criteria for quantifying illness, more exact classifications of treatment, and more long-term studies of their results.

Psychiatrists vary greatly in their understanding. However, understanding is not always correlated with therapeutic effectiveness. There are instances in which a psychiatrist with a warm, sympathetic approach will accomplish much more with a patient than one who sets understanding as his goal. However, I hasten to add that there are some patients who may not only fail to improve with treatment that stems just from the heart—but may actually get worse. In our present state of knowledge, there are some patients who cannot be helped by any treatment short of that which is geared to the uncovering of the unconscious forces that lie behind their illness. Understanding gives a therapist some flexibility in his approach and facilitates the selection of specific goals of treatment for different patients. If a sympathetic approach is adopted, it is for a good reason and not because it is the only form of treatment the psychiatrist has to offer. This is in the direction of being scientific.

We get into difficulty when we speak of "a cause" of emotional disorders. We have learned that they are the result of many forces. We are dealing with things more complicated than infections, and we should be consoled by the fact that it was not too long ago that infectious diseases were not understood and their treatment certainly not specific. We are just at the threshold of an era of psychological medicine, and I most certainly subscribe to Dr. Bowman's plea for recognizing our limitations and ignorance.

## DISCUSSION

ROBERT T. MORSE, M. D., Washington, D. C. —I would like to make a brief comment about the authors' concern with "cure *vs.* meliorism." While in general I agree with them, I think that psychiatrists are unduly sensitive on this score and I ask, does the internist "cure" rheumatic heart disease or diabetes?

While a medical student more than 20 years ago, I was continually impressed by the frequency with which I encountered in a standard textbook of medicine in discussing the treatment of a variety of illnesses ranging from pneumonia to multiple sclerosis, the phrase, "psychotherapy is also helpful." Today I think the situation is not much changed, except that psychotherapy is recommended in an even greater variety of medical contexts. As the authors have pointed out, considerably more is known today about what constitutes psychotherapy in the particular technical sense, but it still lacks the precision of other medical therapies since its quantification is so difficult.

Throughout my professional experience I have been impressed with the wide range of attitudes among physicians, not psychiatrists, regarding psychotherapy as a form of medical treatment—ranging from the situation accurately described by Drs. Bowman and Rose, where it is believed (I hope by only a few) to be a cure-all, and the answer to all manner of human problems, to that of complete rejection of the concept. This skepticism is demonstrated when in earnest conversation the physician often reveals the fact that psychotherapy to him is just another word for advice, counselling, and suggestion, and that psychotherapy at the hands of a psychiatrist is believed to be no different intrinsically from treatment by the family practitioner who advises his depressed patient to "stop worrying, take a trip to Florida, and develop new interests."

For such a physician, the psychiatrist is simply a strange fellow who has the endurance and patience to repeat similar advice for long periods, perhaps interspersed with some remarks or questions about the patient's early emotional life. It is also, unfortunately, from this area that the psychiatrist frequently gets requests for psychiatric consultations. In discussing the patient with the referring physician, it becomes abundantly clear that he has

attempted his own type of "psychotherapy" and has been unsuccessful. In exasperation and frustration, and frequently with evidence of clear-cut hostility for the patient, it becomes apparent that the purpose of the referral is *not* consultation but disposition of a troublesome problem. The referring physician wants the psychiatrist to relieve him of his medical responsibility, and he may actually prefer to hear no more about the matter once treatment is initiated.

I do think it is fair to say that this type of referral more characteristically comes from the older practitioner; the younger man, especially those who have graduated in the last 15 years, when they refer a patient, are genuinely interested in giving full background information and are eager to hear the psychiatrist's clinical impression and to receive progress reports on the treatment if it is undertaken.

As indicated by Drs. Bartemeier and Lebenshon, the psychiatrist would do well to ascertain in advance just what the referring physician expects or hopes for as the happy outcome of psychiatric treatment, and care must be taken that what the referring physician wants is indeed what will best serve the patient.

I agree with all the summary conclusions of Drs. Bowman and Rose, except that which reads: "the training of the modern physician leads him to believe that a 'science of *mind*' exists which in practice can be understood and judged in the same way as can the other branches of medicine."

I do not thiuk this is so; certainly it is not true in those medical schools where psychiatry is taught fully and adequately, which means that the term psychotherapy is understood to mean a specialized and refined procedure in dealing with psychiatric problems and is not to be confused with suggestion, advice, and counselling, important as these procedures are, not only in psychiatry but throughout the practice of medicine.

Drs. Bowman and Rose have vigorously and courageously studied this confused and confusing problem, and I am happy and proud to have been able to discuss their presentation.

# THE ACADEMIC LECTURE

## PSYCHOTHERAPY OF SCHIZOPHRENIA[1]

FRIEDA FROMM-REICHMANN, M.D., Rockville, Md.

When I received the invitation to talk to you about psychotherapy of schizophrenia, I gave a good deal of thought to the question of how you might like me to approach the topic. Finally, I felt it might be most appropriate to report the development in the understanding and the technique of our clinical work since 1948 when I had the privilege to talk to you about it at the schizophrenia symposium during the annual meeting in Washington.

The goal of psychotherapy with schizophrenics was seen then, as it is now, as helping them by a consistent dynamically oriented psychotherapeutic exchange to gain awareness of the unconscious motivations for and curative insight into the genetics and dynamics of their disorder.

As a result of the continued research which is inherent in dynamic psychotherapy, I have gained some further insight into the dynamics of schizophrenic symptomatology from which have evolved some variations in the details of the treatment. Briefly, they are:

1. The old hypothesis according to which the schizophrenic's early experiences of warp and rejection were of over-all significance for the interpretive understanding and treatment has been somewhat revised.

2. The conflict-provoking dependent needs of schizophrenic patients have been seen more clearly.

3. The devastating influence of schizophrenic hostility on the patients themselves has been understood more clearly in connection with their states of autism and partial regression (weak ego—autistic self-depreciation).

4. This has led to a therapeutically helpful reformulation of the anxiety of schizophrenic patients as an outcome of the universal human conflict between dependency and hostility which is overwhelmingly magnified in schizophrenia.

5. The multiple meaning of some schizophrenic communications and its influence on the psychiatrist's interpretive endeavors has been clarified.

Before I begin to elaborate these topics, I have to ask you to forgive me for lack of reference to publications of other workers in the field. There is unfortunately not time enough to comment on the published work of our colleagues, to indicate what I owe to them, and also to develop my own conceptions. So, I felt I ought to decide to do the latter.

I would like to begin by stating that my discussion will comprise the treatment of hospitalized disturbed psychotics as well as that of manifestly less disturbed ambulatory patients whom we treat in the same way through all phases and all manifestations of their illness. This position is not new, but it has recently become more controversial due to opposite techniques which other authors have propagated.

From a social and behavioral standpoint and from the viewpoint of the special care which manifestly psychotic patients may need in order to be protected from harming themselves and others, the difference between these two types of patients may seem tremendous. Psychodynamically speaking, I see no difference between the symptomatology of actively psychotic and more conformative schizophrenics.

All schizophrenic patients live in a state of partial regression to early phases of their personal development, the disturbed ones more severely regressed than the conformative ones. All of them are also living simultaneously on the level of their present chronological age, the conformative ones more obviously so than the severely disturbed

[1]Read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954. The three papers that follow are discussions of this lecture.

ones. Irrespective of the degree of regression and disturbance, we try to reach the regressed portion of their personalities by addressing the adult portion, rudimentary as this may appear in some severely disturbed patients. Also, the general psychodynamic conception that anxiety plays a central role in all mental illnesses and that mental symptoms in general may be understood simultaneously as an expression of and as a defense against anxiety and its underlying conflicts holds regardless of the severity of the picture of illness, and regardless of its more or less dramatic character. Hence we make the exploration of the dynamic roots of the schizophrenic's anxieties our potential goal through all phases of illness.

Lack of immediate communicative responses to treatment in acutely disturbed patients is no measuring rod for their actual awareness of and for their inner response to our psychotherapeutic approach. This old experience has been further corroborated in more recent dealings with several recovered patients. They did refer to various aspects of our psychotherapeutic contacts, after their emergence, while we were working through the dynamics of their problems, or later while we were reviewing treatment and illness during the recovery period.

While symptomatic psychotherapy of acute psychotic manifestations may be necessary with some patients, for situational reasons, many of us consider it not too important to be overconcerned with the duration of the acutely disturbed states of patients while they are under psychotherapy.

My experience during the last 20 years has been mainly with schizophrenic patients who came to our hospital in a state of severe psychotic disturbance, from which the majority emerged sooner or later under intensive dynamic psychotherapy. After their emergence, they continued treatment with the same psychiatrist through the years of their outwardly more quiet state of illness, with the aim of ultimate recovery with insight. During both phases the patients were seen for 4 to 6 regularly scheduled weekly interviews lasting one hour or longer. Sometimes relapses occurred. Such relapses were due to failure in therapeutic skill and evaluation of the extent of the patient's endurance for psychotherapy, to unrecognized difficulties in the doctor-patient relationship, or to responses to intercurrent events beyond the psychiatrist's control. As a rule, these relapses could be handled successfully if the psychiatrist himself did not become too frightened, too discouraged, or too narcissistically hurt by their occurrence.

From the experience with these patients we learned about one more reason for advocating the same type of psychotherapeutic approach through all phases of the illness: part of the work which a patient has to accomplish during treatment and at the time of his recovery is, in my judgment, to learn to accept and to integrate the fact that he has gone through a psychotic illness, and that there is a "continuity," as one patient called it, between the person as he manifested himself in the psychosis and the one he is after his recovery. The discussion of the history of patients' illness and treatment after their recovery serves of course the same purpose. This is in contrast to the therapeutic attitude of some psychiatrists who hold that recovering patients should learn to detest and eject their psychotic symptomatology, like a foreign body, from their memory.

The difficult task of integrating the psychotic past, which we advocate, will be greatly facilitated if it can be done on the basis of patient's memory of a psychiatrist who has maintained the same type of psychotherapeutic relationship with them through the whole course of treatment. Changes in the doctor's therapeutic approach may easily become a mirror of the lack of continuity in the patient's personality, and, incidentally, may become an inducement for patients to dwell in one or the other phase of their illness, depending upon their preference for this or the other type of therapeutic relationship.

The following experience with a patient illustrates the difficulties of integrating the experience of a past psychosis.

This patient emerged from a severe schizophrenic disturbance of many years duration, for which she was finally hospitalized for 2 years at Chestnut Lodge and then treated as an ambulatory patient for another 2 years. Eventually she became free of her psychotic symptomatology except for the maintenance of one manifest symptom: she would hold on to the habit of pulling the skin off her heels

to the point of habitually producing open wounds. No attempt at understanding the dynamics of this residual symptom clicked, until the patient developed one day an acute anxiety state in one of our psychotherapeutic interviews in response to my commenting on favorable "changes" that had taken place in her. After that, the main dynamic significance of the skin-pulling became suddenly clear to her and to me. "I am still surprised and sometimes a little anxious about the change which I have undergone," she said, "and about finding and maintaining the continuity and the identity between the girl who used to be so frightfully mixed up that she had to stay locked up on the disturbed ward of Chestnut Lodge, and the popular and academically successful college-girl of today." The skin-pulling as a symptom similar to another self-mutilating act of burning herself, which she repeatedly committed while acutely ill, helped her to maintain her continuity. It made it possible to be ill and well at the same time, because it was only she who knew about the symptom which could be hidden from everybody else with whom she came in contact as a healthy person. After this discovery, the symptom eventually disappeared.

Incidentally, important as the understanding of this one dynamic aspect of the patient's symptom was for therapeutic reasons, this does not mean that it constituted its only significance.

It was stated that mental symptoms in general can be understood as a means of expressing and of warding off anxiety and the central conflicts which are at the root of this anxiety, and that the exploration of this anxiety is most important in psychotherapy with schizophrenics. If this is true, we have to ask for a specific psychodynamic formulation of the causal interrelatedness between schizophrenic symptomatology and the conflicts underlying the anxiety in schizophrenic patients. A correct workable conception of the psychodynamic correlation between anxiety and schizophrenic symptom-formation is a prerequisite for the development of a valid method of dynamic psychotherapy with schizophrenic patients.

We know the historically determined deadly fear of schizophrenics of being neglected, rejected, or abandoned, and their inability to ask for the acceptance and attention they want. Consequently, most psychiatrists who did psychotherapy with schizophrenics in the early days suggested treating them with utter caution, as I did, or with unending maternal love, permissiveness, and understanding as did Schwing and more recently Sechehaye. While doing so, psychiatrists faced another dynamically significant problem of the schizophrenic, the unconscious struggle between his intense dependent needs and his recoil from them. These we learned to understand genetically as the correlate to the patients' experience of neglect by the "bad mother" at a time when her attention was indispensable for the infant's and the child's survival.

We also know about the resentment, anger, hostility, fury, or violence, with which the infant and child, the "bad me" as Sullivan called it, and later the schizophrenic patient, responds to the early damaging influences of the "bad mother," as he experienced her.

In order to understand the devastating significance of this hostility for schizophrenic patients, we have to realize the following developmental facts of their lives. As we first learned from Freud and Bleuler, schizophrenics are people who responded to the early misery of their interpersonal contacts not only with anger and hostility, but also with a partial regression into an early state of ego-development and of autistic self-concern and self-preoccupation. This early traumatization and the partial regression make for a weak organization of the schizophrenic's ego. Consequently, he feels more threatened than other people by all strong emotional experiences, and above all, by the realization of his own hostile impulses.

Another reason for the specific hardship which schizophrenic hostility creates for the patients is that their autistic self-preoccupation makes for their being painfully concerned with their own "bad me," with their own hostility and fury, or their fantasies of violence and destruction against themselves and others.

Besides, their grandiose concept of power in these states of regression to an early state of interpersonal development makes for their preoccupation with themselves as more or less dangerous people.

Where other types of patients are mainly concerned with the fear of disapproval, of the withdrawal of love which they may elicit in other people by their hostile impulses or other emanations of their "bad me," schizophrenic patients are more concerned with their own status as dangerously hostile peo-

ple, with the damage which may be done to others who associate with them, and with their impulses of punitive self-mutilation.

Yet, neither the fearful and grandiose self-preoccupation with his dangerous hostility, nor the threat of the primary abandonment by mother, nor the resulting dependent needs from which the patient simultaneously recoils, nor the secondary rejection he may have elicited in the mother and other significant persons in his environment because of his "badness" are in themselves potent enough to elicit schizophrenic anxiety.

Schizophrenics suffer, as all people in our culture do even though to a much lesser degree, from the tension between dependent needs and longing for freedom, between tendencies of clinging dependence and of hostility. For the above-mentioned reasons the degree of the schizophrenic's need for dependency, the extent to which he simultaneously recoils from it, and the color and degree of his hostile tendencies and fantasies toward himself and others are much more intense than in other people. As a result, the general tension engendered by the clash of each of these single powerful emotional elements becomes completely overwhelming. In other words, the quantitative difference between the schizophrenic's anxiety and similarly motivated tensions in people who have not been emotionally traumatized as early in life as the schizophrenic, and who could therefore develop a stronger ego organization, is so great that it acquires a totally different quality. It is this tremendous volume of the schizophrenic's anxiety which makes it unbearable in the long run. It then has to be discharged by symptom-formation; *i.e.*, schizophrenic symptomatology is seen as the expression of and defense against schizophrenic anxiety, engendered by the tremendous tension between his great dependent needs, his fear to give them up, his recoil from them, his hostility, his thoughts and fantasies of destructiveness against himself and others.

In delineating the dynamic interrelatedness between schizophrenic anxiety and symptomatology, I do not claim, of course, to solve the total problem of schizophrenic symptomatology. I am referring only to such portions of the dynamics as seems necessary for the clarification of my therapeutic conceptions. Our treatment of many schizophrenic manifestations has been corrected or markedly improved in the light of the hypothesis offered.

Take for example the meaning of the schizophrenic's "fear of closeness," a formulation which, incidentally, has been much abused. In the early years of psychotherapy with schizophrenics we used to understand this fear of intimacy as an expression of anxiety that all closeness, much as it was simultaneously desired, might be followed by subsequent rejection; then we learned that this fear of closeness seemed also strongly determined by the fear which the partially regressed schizophrenic with his weak ego-organization felt, that closeness might endanger his identity, might destroy the boundaries between his own ego and that of the other person.

In the meantime, I learned from my work with quite a number of further patients, that their fear of closeness is tied up with their anxiety regarding the discovery of their secret hostility or violence against persons for whom they feel also attachment and dependence. They give a mitigated, non-dangerous expression to this hostility, and try simultaneously to hide it as a secret by staying away from people.

Let me mention, in this context, an experience which I had repeatedly with patients whom I saw in an office connected with my home: they became tense and anxious when we met after my secretary and maid had left the house. The patients commented on the lack of protection against their hostile impulses.

One young paranoid patient formulated this outrightly, by asking, "Do you realize that I can knock you down in no time?" Unfortunately, I became preoccupied with my role of demonstrating the lack of fear which at the time was luckily mine. Thus, I failed to notice how frightened the patient felt by the realization of his potential violence against a woman doctor, with whom he had established at the same time a dependent relationship. Later on I realized that he was warning me against and asking for protection from future acts of violence, by which he felt we were both threatened. Subsequently, such threats against me or other doctors whom he accidentally saw in my house, against the house itself, and against the attendants who came to take care of him, were the unfortunate

result. All these assaultive acts were accompanied by marked signs of anxiety.

I continued seeing the patient in a wet pack, until he agreed to abstain from all violent actions and to express his hostile feelings verbally. This he did for some time, alternately with verbal expressions of his dependent attachment and with nonverbal signs of anxiety, until he developed a marked manifest psychotic symptomatology. Since then, it became more difficult to have the patient face his dependent needs and his hostility or the anxiety engendered by both. Had I caught on immediately to the patient's anxiety regarding his own hostility, he might have been spared the necessity of transforming it into overt psychotic symptomatology.

Let us now take a look at states of catatonic stupor in the light of our hypothesis. I believe it is of interest to state that many clinicians have been accustomed to describe stuporous states as a result of the schizophrenic's withdrawal of interest from outward reality. Hence the oversimplification of interpreting them only as a response to catatonic fear of rejection becomes quite understandable.

Actually, a patient in stupor has not withdrawn his interest from the environment. As we know from reports about the experiences while in stupor, which these patients furnish after their emergence, they are, more frequently than not, keen observers of what is going on in their environment. Withdrawal of the ability for interpersonal communication is what characterizes the condition of the patient in stupor, not withdrawal of interest in the environment *per se*. As we know now, this comes about not only in response to the threat of rejection by others, but much more for fear of the patient's own hostility or violence in response to actual or assumed acts of rejection from other people.

I remember in this connection the catatonic patient previously reported who became stuporous when she did not receive my message that I had to postpone a scheduled interview. Upon discovering this unfortunate omission, I painstakingly explained the situation to the patient. When she heard and understood me, she emerged from the stuporous state and psychotherapeutic contact could be resumed.

Incidentally, while telling you about my therapeutic approach to this or other patients, I have to fight off a temptation to dramatize; this in spite of the fact that dramatization does certainly not go with what I would consider good taste in delivering a scientific paper. Upon asking myself about the reason for this temptation, I discovered that actually it is not as illegitimate as it appears to be. It is promoted by the fact that I feel inclined to duplicate tone and inflections of the patient's and my voice, the concomitant gestures, changes in facial expression, etc. This comes about because the doctor's nonverbal concomitants of the psychotherapeutic exchange with schizophrenic patients, in and outside of manifestly psychotic episodes, are equally if not at times more important than the verbal contents of our therapeutic communication.

The particular emotional stimulus to which a stuporous schizophrenic will respond, which instigated this digression, must be much stronger than one that can be produced by the content *per se* of what is said. An academic type of delivery to the patient will not do the trick.

Of course, to a certain extent nonverbal elements play a great role in all interpersonal communications, but the degree of expressive skill with which the patient himself uses means of nonverbal communication, and his specific sensitivity to the meaning of its use by the psychotherapist is such that for all practical purposes the difference in quantity, here again, turns actually into one of quality.

This great perceptive sensitivity of schizophrenic patients was one of the reasons for my overcautious approach to them in bygone times. We used to look at the sensitiveness of these patients in a merely descriptive way and labelled it as one of their admirable characteristics. If we investigate it psychodynamically we realize that it develops actually in response to their anxiety as a means of orientation in a dangerous world, and we can use it as a signpost on our road toward the psychodynamic investigation of schizophrenic anxiety. Also we should not overlook the possibility that many of the initially correct results of the schizophrenic's perceptive sensitivity may be subsequently subject to distorted psychotic interpretation and misevaluation.

To return to our discussion of the psychodynamics of states of catatonic stupor, I too used to interpret them as a sign only of the

patients' having withdrawn because of the lack of consideration or rejection of them. I believe now that this is neither the primary nor the only cause, and that withdrawal into stupor is more strongly motivated by the anxiety of patients who realize the danger of their own hostile responses to such neglect by people on whom they depend and to whom they are attached. Several patients corroborated the validity of this hypothesis by spontaneous comments after their recovery.

The symptoms that patients in stupor show concomitant with their withdrawal of interest from communication furnish another proof. Stuporous patients regress to a period of life when they used food-intake and elimination as an expression of their hostility against and of their wish to exert control over their environment.

The hostile meaning of disturbances in elimination can also be demonstrated outside of stuporous states. I had impressive proof of it in my dealings with a schizophrenic woman patient, who is also mentioned in the Stanton and Schwartz paper, "A Social Psychological Study of Incontinence."

One day, this patient urinated, before I came to see her, on the seat of the chair on which I was supposed to be seated during our interview. I did not see that the chair was wet. The patient did not warn me and I sat down. I became aware of the situation only after the dampness had penetrated my clothing. I thereupon expressed my disgust in no uncertain terms. Then I stated that I had to go home. The patient asked anxiously about my coming back, which I refused with the explanation that the time allotted to our interview would be over by the time I would have taken a bath and attended to my soiled clothes.

Obviously, the patient's wetting my chair was an expression of hostile aspects in her dependent relationship with me. However, I did not say so in so many words, because I felt that the verbalization of this insight should come from the patient. In subsequent discussions of the event, she responded first with symptom-formation and nonverbal communication, wavering back and forth from expressions of hostility against me to expressions of attachment and dependence, until she was finally able to reveal that this had been a planned expression of resentment against me. The patient wished to punish me for what she had experienced as excessive therapeutic pressure during an interview preceding the chair-wetting.

Certain symptoms of several hebephrenic patients of our observation could also be psychodynamically understood and therapeutically approached as an expression of the anxiety connected with their hostility toward people on whom they likewise felt extremely dependent. These patients withdrew their interest from their interpersonal environment except for a kind of tolerant and peaceful, if incomprehensible, give-and-take with some of their fellow patients, until it all was suddenly interrupted by an outburst of hostility against these patients or against the personnel. As far as their dealings with me went, they did what hebephrenic patients will do at times, as we all know: a kind of mischievous smile or laughter accompanied or interrupted their scarce communications or was in itself the only sign of their being in some kind of contact with me. Two patients stated, after they were ready to resume verbal contacts with me, that their laughter was a correlate of hostile derogatory ideas against and fantasies about me. As they at last established a close relationship of utter dependence upon me, this was accompanied by a marked increase in intensity and duration of these spells of derogatory, tense laughter. The anxiety connected with the establishment of a dependent relationship expressed itself and was warded off by the increased derogatory laughter. The laughter subsided eventually, in response to the psychotherapeutic investigation and the working through of the various aspects of the patients' relationship with me.

With regard to paranoid patients, one of their dynamisms is, as we know, that they project onto others the blame for what they consider blameworthy in themselves. Upon investigation of the contents of their blameworthy experiences we always discover that they are extremely hostile in nature. The suspiciousness of these people points in the same direction.

Again, their suspicion and hostility increase parallel with the realization of their friendly dependent relationship with the psychiatrist. This showed quite impressively in the above-mentioned violent man patient. The fact that the office where we initially met was part of my home became to him, to use Mme. Sechehaye's expression, a "symbolic realization" of his wish to be my friend and houseguest. As he fantasied that

I shared his wishes and hallucinated that he heard me say so, he became more and more hostile and anxious.

If our hypothesis about the interrelatedness between craving for and recoiling from dependency, dangerous hostility and violence against themselves and others, overwhelming anxiety and schizophrenic symptomatology is correct, we must ask how the therapeutic approaches of consistent love and permissive care, as they used to be given to schizophrenic patients by some therapists, including myself, could be helpful. We used to think that they were successful (1) because they gave a patient the love and interest he had missed since childhood and throughout life; (2) because his hostility could subside in the absence of the warp which had originated it; and (3) because the patient was helped to re-evaluate his distorted patterns of interpersonal attitudes toward the reality of other people.

We now realize that what we have long known to be true for neurotic patients also holds true for schizophrenics. The suffering from lack of love in early life cannot be made up for by giving the adult what the infant has missed. It will not have the same validity now that it would have had earlier in life. Patients have to learn to integrate the early loss and to understand their own part in their interpersonal difficulties with the significant people of their childhood.

I also know now, and can corroborate this with spontaneous statements of recovered patients, that the love and consideration given to them is therapeutically more significant because they interpret it as proof that they are not as bad, as hostile in the eyes of the therapist, as they feel themselves to be.

The few fragments of therapeutic exchange with patients quoted so far may serve as examples of the change in our psychotherapeutic attitude, part of which I already elaborated in my contribution to the 1950 Yale Symposium on Psychotherapy with Schizophrenics.

Of course, we give our schizophrenic patients all the signs of empathic consideration that they need because they suffer. If possible, we prefer to do so by implication or in nonverbalized innuendoes. Too marked sympathetic statements may enhance fear of intimacy and they may unnecessarily increase patients' dependence on the therapist, putting into motion the psychopathological chain of dependent attachment, resentment, anxiety, symptom-formation.

However, we no longer treat the patients with the utter caution of by-gone days. They are sensitive but not frail. If we approach them too cautiously, or if we do not expect them to be potentially able to discriminate between right and wrong, we do not render them a therapeutically valid service. We contribute to their low self-evaluation, instead of helping them to develop a healthier attitude toward themselves and others.

Also, if there was lack of parental interest in infancy, this entails lack of guidance in childhood. This fact deserves more therapeutic consideration than it has been given so far. There are therapeutically valid variations of the guidance needed and missed in early childhood, which can be usefully included in psychotherapy with schizophrenics in adulthood.

One exuberant young patient, the daughter of indiscriminately "encouraging" parents, was warned against expecting life to become a garden of roses after her recovery. Treatment, she was told, should make her capable of handling the vicissitudes of life which were bound to occur, as well as to enjoy the gardens of roses which life would offer her at other times. When we reviewed her treatment history after her recovery, she volunteered that this statement had helped her a great deal, "not because I believed for a moment that you were right, doctor, but because it was such a great sign of your confidence in me and your respect for me, that you thought you could say such a serious thing to me and that I would be able to take it."

In line with our attempts at raising patients' low opinion of themselves, we replace offers of interpretations by the therapist, if possible, by attempts at encouraging patients to find and formulate their interpretations themselves, as demonstrated in my exchange with the patient who wet the chair.

So far we have discussed the psychodynamics of schizophrenics symptom-formation in general as a response to their anxiety. Let us now consider the double and multiple meaning that is inherent in many of the schizophrenic's cryptic and distorted manifestations. Many of them elude the psychiatrist's understanding, but they may yield indirectly to therapeutic endeavors in

other areas. Insight into their dynamics may thus be gained in subsequent discussions.

Others, such as hallucinations and delusions, I found frequently accessible to a direct psychotherapeutic approach. They would be successfully examined with the patient as they occurred in his experience and in terms of his own formulations. I stated, however, explicitly to the patient that I did not share his hallucinatory or delusional experience.

There is one more access to understanding schizophrenic communications which has not been mentioned as yet. Schizophrenics are able to refer in their productions simultaneously to experiences from the area of their early childhood, from their present living in general, and, if they are under treatment, from their relationship with the therapist, like dreamers do in their dreams. Sometimes we are able to understand the meaning of and their reference to various chronological levels of the patients' experience, sometimes not.

At any rate, it is most important for the psychiatrist to realize this multiple meaning of many schizophrenic symptoms and communications. This realization should make us replace the old therapeutic attitude that therapists ought to be able to find and offer to the patient the only correct meaning of a symptom or communication by the suggestion that they should train themselves to become able to feel which of several meanings of a schizophrenic symptom or communication (if they catch on to several of them) is the therapeutically most significant one at a given time. This ability of the psychiatrist to select sensitively when and what to present to the patient is most desirable, because of the narrowed ways of the schizophrenic's thinking and their short span of attention which limits their capacity to listen.

The insights into the possibilites and the limitations of understanding schizophrenic communications should do away with the endless discussion that used to go on between various members of groups of psychotherapists as to whether a patient's communication in word or action meant only what Dr. A. heard or exclusively what Dr. B. heard. Depending upon the scope of personal and clinical experience and the personality of the therapist and on his ability to understand patients' communications via identification, each among several psychotherapists may catch on to one of the different meanings of a patient's communication.

The insight into the manifold meanings of patients' symptoms or other manifestations may also do away with the continuing discussions in our literature of the question whether or not schizophrenic patients understand their own communications. I believe it should be stated that they sometimes do and sometimes do not. Sometimes they may, above all, be aware of the descriptive content of their communication, but not of its dynamic significance. While this whole question holds great theoretical interest, I believe now that for therapeutic purposes its solution is not too important. This holds true all the more since the main trends in treatment no longer go in terms of translating the descriptive meaning of the content of any single symptom.

There are two facts that have led us more and more away from working with patients in terms of interpreting their various symptoms and other cryptic communications. One is negative and is determined by the fact that most isolated interpretations of the content of a single symptom or other communication will not cover all its meanings in a therapeutically significant way. The other is an important positive one: it follows from the knowledge of the psychodynamic fact that schizophrenic patients, like any other mental patients under treatment, repeat with the therapist the interpersonal experiences which they have undergone during a lifetime.

Hence we have moved increasingly in the direction which I have already elaborated in previous papers: we make the therapeutic exploration and clarification of schizophrenic anxiety and symptomatology, as they manifest themselves in the patient-doctor relationship, as integral a part of psychotherapy with schizophrenics as it is with neurotic patients. Some modifications are, of course, required in view of the difference between schizophrenic and neurotic modes of relatedness with the psychiatrist and with other people. But in both cases, our therapeutic attention is focused on the dynamic investigation and clarification of the conscious and the un-

conscious aspects of the patient-doctor relationship in its own right and in its transference aspects. Special attention is paid to the exploration of the anxiety aroused by the therapist's probing into the patients' problems, and to their security operations against it.

Here is an example from the treatment history of the patient who pulled the skin off her heels, which illustrates both the multiple meaning of schizophrenic symptoms on various experiential levels and our approach to its basic dynamic significance in terms of investigating its manifestations in the patient-doctor relationship:

We are already familiar with the dynamic validity of the skin-pulling as a way for the patient to establish her "continuity." As we learned in the course of its further investigation, the localization of this symptom was determined by mischievously ridiculing memories of her mother's coming home from outings to prepare a meal for the family, going into the kitchen, removing shoes and stockings but not coat and hat, and walking around the kitchen on bare feet.

The self-mutilating character of the symptom proved to be elicited by the patient's resentment against me. In her judgment, I misevaluated the other act of self-mutilation from which she suffered during her psychotic episodes, the compulsion of burning her skin. The patient thought of them as a means of relieving unbearable tension, whereas she felt that I thought of them only as a serious expression of tension. In maintaining the skin-pulling, while otherwise nearly recovered, she meant to demonstrate to me that skin injuring was not a severe sign of illness.

During the treatment period after the dismissal from the hospital, the patient tried for quite a while to avoid the recognition of her hostility against me and the realization of her dependent attachment to me which she resented, by trying to cut me out of her every-day life. She did so, repeating an old pattern of living in two worlds, the world which she shared with me during our therapeutic interviews, and life outside the interviews, during which she excluded me completely from her thinking. Previously, the patient had established this pattern with her parents by living for 11 years in an imaginary kingdom which she populated by people of her own making and by the spiritual representations of others whom she actually knew. They all shared a language, literature, and religion of her own creation. Therapeutic investigation taught us that the patient erected this private world as a means of excluding her prying parents from an integral part of her life. It was her way of fighting her dependence on them and of demonstrating how different she was from them in all areas where she disliked and resented them.

The patient recognized the significance of the dichotomy in her dealings with me as a means of escape from her resentment against and dependence on me, only after going twice through a sudden outburst of hostility and anxiety which led to brief periods of re-admission to the hospital where she regressed to her old symptom of burning herself.

After a few stormy therapeutic interviews, she understood the dynamic significance of her need for readmission; she felt so dependent on me and so hostile against me that she had to come back to live in the hospital and to burn her skin.

During the ambulatory treatment periods which followed, the patient learned eventually to recognize that her excluding me from one part of her life was a repetition of the exclusion of her parents from her private kingdom. After that, she saw too that her resentment against me was also a revival of an old gripe against her parents; they had a marked tendency to make her out to be dumb, as I tried to do, in her judgment, by putting over her my misevaluation of the skin burning. They kept her for many years in a state of overdependence, as I had done too, by virtue of our therapeutic relationship.

All these transference facets of the patient's relationship with me, as well as the problems of the doctor-patient relationship in their own right had to be worked through several times before the patient could ultimately become free from her interpersonal difficulties with me, with her parents and other people, and from the anxiety which they engendered.

While we consider the suggestions about psychotherapy with schizophrenics, which we have offered, to be psychodynamically valid and helpful rules, we believe, on the other hand, that the ways and means to go about using them will be inevitably subject to many variations, depending on the specific assets and liabilities of the personality of the therapist, and, hence, on the specific coloring of his interaction with his patient.

Psychotherapy with schizophrenics is hard and exacting work for both patients and therapists. Every psychiatrist must find his own style in his psychotherapeutic approach to schizophrenic patients. About technical details such as seeing patients only in the office, walking around with them, seeing them for nonscheduled interviews I used to have strong feelings and meanings. Now I consider them unimportant, as long as the psychotherapist is aware of and alert to the dynamic significance of what he and the patient are doing, and what is going on between

them. What matters is that he conducts treatment on the basis of his correct appraisal and exploration of the psychodynamics of the patient's psychopathology and its manifestations in the doctor-patient relationship. Successful histories of treatment with the principles suggested, but conducted in various and sundry interpersonal and environmental settings, are a living proof of the validity of my present corrected attitude.

Since the work with schizophrenics makes great and specific demands on the psychiatrist's skill and endurance, no discussion of psychotherapy with schizophrenics is satisfactory as long as the consideration of the specific personal problems of the therapist is omitted. In view of the extensive previous discussions of this topic by others and by myself, I shall only briefly enumerate the specific problems and requirements which ought to be met and solved by psychiatrists who wish to work with schizophrenics: they should be able to realize and constructively handle unexpected emotional responses, such as fears or anxieties, at times inevitably aroused in each of them by anxious, violent, overdependent, or lonely schizophrenic patients.

There is one special point I might add. Psychotherapists who share the fear of loneliness, which is the fate of men in our time, must watch out specifically lest their need to counteract their own loneliness make them incapable of enduring the inevitable loneliness and separation that their schizophrenic patients may bring home to them in their isolating cryptic communications. An undesirable urge to translate cryptic schizophrenic communications prematurely may interfere in such therapists with the more sound tendency to patiently wait and listen to the patients' own explanations of their communications.


## Summary

1. The goal of dynamic psychotherapy with schizophrenics is the same as that of intensive psychotherapy with other mental disturbances, i.e. to help both ambulatory and hospitalized patients gain awareness of and curative insight into the history and unknown dynamic causes which are responsible for their disorder.

2. The same type of psychotherapeutic approach to schizophrenic patients during all phases and manifestations of the disorder and discussions of illness and treatment after their recovery are recommended for the purpose of helping such patients to integrate their recovery with their psychotic past.

3. An attempt is made to understand schizophrenic symptomatology and to approach it therapeutically as an expression of and as a defense against anxiety. The hypothesis is offered that the universal human experience of tension between dependency, fear of relinquishing it, recoil from it, and interpersonal hostility becomes, in the case of schizophrenic persons, so highly magnified and so overwhelming that it leads to unbearable degrees of anxiety and then to discharge in symptom-formation.

4. The multiple meaning of many schizophrenic symptoms, communications, and other manifestations has been discussed. The need for understanding and translating them descriptively for therapeutic reasons has been questioned, and the significance of nonverbal communications with schizophrenic patients has been stressed.

5. Psychodynamic investigation and clarification of schizophrenic anxiety and symptomatology in its conscious and unconscious manifestations in the patient-psychiatrist relationship is presented to be equally as crucial for the psychotherapy with schizophrenics as for other mental patients.

# I. HATEFUL SELF-DISTRUST: A PROBLEM IN THE TREATMENT OF SCHIZOPHRENIC PATIENTS[1]

JOHN C. WHITEHORN, M.D., BALTIMORE, MD.

Dr. Fromm-Reichmann has presented a brilliant and challenging statement of her current thinking about schizophrenic patients and their treatment. More than in previous publications, she has stressed the central significance of the schizophrenic patient's self-distrust and his dread of his own destructive impulses. She has provided some illuminating clinical examples of the patience and understanding required of the therapist by reason of this particular obstruction to communication and interaction.

In contrast to Dr. Fromm-Reichmann's way of working there has been a type of psychoanalytic effort, characteristic of doctrinaire psychoanalysts, to find and offer to the patient *the correct interpretation* of a symptom or statement, as if that were the effective means of therapy. Dr. Fromm-Reichmann, after long and intensive experience, tells us of her skepticism as to the effectiveness of that approach. In my judgment she shows great wisdom in recognizing, instead, a potential multiplicity of meanings in a given symptom or communication. She states, and I agree with her thoroughly, that each among several psychotherapists may catch onto one of the different meanings of a patient's action, and utilize such understanding more or less helpfully, depending upon differences in the personality of the therapist and the scope of his personal and clinical experience. She says, and I quote: "No isolated interpretation of a single symptom or other communication will cover all its meanings in a therapeutically significant way."

On this point, as on many points, I find a very large measure of agreement between my own views of schizophrenic patients and their psychotherapeutic treatment[2] and those just presented by Dr. Fromm-Reichmann. This is not a surprise. Except for tactics and technical detail, we have, over a number of years, perceived much agreement. As Dr. Fromm-Reichmann's extensive and intensive experience has modified her approach and enabled her to subordinate the tactics and the specific hypotheses of the original Freudian approach, I note with great pleasure an increasing convergence toward what appears to be a practical identity in our formulations of the central problem of the psychotherapy of schizophrenic patients. Dr. Barbara Betz and I have just finished writing a paper, completing one phase of a rather prolonged study of psychotherapeutic relationships between physicians and schizophrenic patients. In the general introduction to that paper[3] we have said:

> In the early years of this century the possibility that schizophrenic patients might actually become engaged in a meaningful personal relationship, with therapeutic benefit, was not widely credited. The schizophrenic patient is not, however, absolute in his inhospitality to overtures from others. The meaning of the social distance maintained by the schizophrenic patient has become increasingly intelligible as a sensitive interpersonal pattern of separateness, motivated *by a fearful and hateful lack of faith in himself and others.*

This statement seems to me practically identical, in substance, with statements just made by Dr. Fromm-Reichmann.

I wish now to comment upon two specific points. (1) In the analysis of a specially selected sample of our Phipps Clinic experience, covering 14 physicians and 100 of their schizophrenic patients in the years 1944-52, Dr. Betz and I have noted favorable responses to psychotherapy in a majority of our patients, but not in all. There are systematic differences in responsiveness to therapeutic efforts, which our study leads us to

[1] This and the two following papers are discussions of Dr. Frieda Fromm-Reichman's Academic Lecture, read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954, and which appear on page 422 *et seq.* of this issue.

[2] See: Whitehorn, John C. Psychodynamic considerations in the treatment of psychotic patients. U. of W. Ontario Med. J. 20: 27, 1950; and Whitehorn, John C. Psychodynamic approach to the study of the psychoses, Chapter IX in *Dynamic Psychiatry*, Alexander and Ross, U. of Chicago Press, 1952.

[3] Am. J. Psychiat., 111, 321, Nov. 1954.

attribute to differences in the approach of the therapist. Differences in approach are, for example, manifested in that portion of our clinical records which we call the "Personal Diagnostic Formulation." There is a considerable degree of freedom for our staff in their statements of personal diagnostic formulations (the only strict rule being that one does not run a formulation beyond one typewritten page). Some doctors quite naturally include motivational terms, and express their understanding of the patient in a basically motivational interpretation. Others limit themselves to mere description, or to a strictly narrative type of biographical statement. Those schizophrenic patients whose therapists approached their treatment with personal diagnostic formulations expressed in motivational terms showed a significantly better improvement rate than did those whose therapists formulated no motivational meanings in their personal diagnostic formulations. We are not, at this point, dealing with the correctness or the incorrectness of these motivational interpretations, only stating in a comparative way that the doctors whose schizophrenic patients do well are those who naturally attempt some motivational understanding, utilizing *some* of the multiple meanings found in the schizophrenic patient's clinical manifestations and behavior.

(2). Dr. Betz and I have also delineated a type of doctor-patient relationship which we call "active personal participation," characterized by realistic flexible interaction, the use of initiative in sympathetic inquiry, the expression of honest disagreement, the challenging of patients' self-deprecatory attitudes, the setting of realistic limits to what is acceptable in patients' behavior, and the avoidance by the doctor of getting caught permissively, so to speak, in the patient's obsessive-compulsive patterns of control and manipulation. These characteristics are recognizable as manifestations of an attitude of respectful and sympathetic independence, on the part of the doctor toward the patient, combined with an expectation that the patient also has potentiality for respectful independent action, and that neither patient nor doctor needs to submit to the other. This is an attitude difficult for schizophrenic patients to believe at first. Our comparative study has shown that the patients whose therapists undertook and maintained this pattern of personal participation showed significantly better improvement rates than those whose therapists attempted to relate to them by other tactical approaches, such as a passive permissive pattern, practical care only, or detailed interpretation and instruction.

These two points have been selected for emphasis here from a number of conclusions drawn by Dr. Betz and myself in a study of the course and outcome of 100 schizophrenic patients whose principal psychotherapeutic treatment had been conducted not by ourselves but by certain other members of our staff. In making this type of analysis and comparison of variants in therapeutic approach, we have gained a methodological advantage, not available when one worker compares his own results over a long term of years. It is difficult for one physician to plan and maintain crucially different approaches and attitudes and patterns of interaction, for the sake of planned comparisons, because some very significant variables may be manifestations of the therapist's own personality characteristics, not readily changed by planned effort. An analysis and comparison of the differences between different physicians and their different styles of transactions with schizophrenic patients has provided for us a better opportunity for revealing the differential effects on patients' progress and outcome. The two points which I have presented illustrate fairly well how closely our conclusions fit with the general viewpoint presented by Dr. Fromm-Reichmann. There are some points of apparent disagreement, particularly regarding the therapeutic significance of interpretations and insight, which seem to me relatively inconsequential.

# II. THE PSYCHOPATHOLOGIC BASIS OF PSYCHOTHERAPY OF SCHIZOPHRENIA[1, 2]

OSKAR DIETHELM, M.D., NEW YORK CITY

Treatment must be based on a dynamic psychopathology which considers the biosocial individuality of the patient. It is therefore necessary that one establish the psychopathologic phenomena in their dynamic significance, their relationship to each other and to physical, psychologic, and social factors. It is important that one observe the changing psychopathology during the whole period of treatment and try to separate essential from accessory or even incidental symptoms. Our concepts of essential and accessory symptoms have changed greatly during the last few decades. It is necessary to look critically at the descriptions and formulations of the earlier workers, *e.g.*, Eugen Bleuler and Adolf Meyer. In American literature, it is rarely mentioned that Bleuler spoke not of schizophrenia but of the group of schizophreniae. His original concept implied that the broad group of schizophrenic illnesses should be reconsidered with progress in our psychopathologic and clinical knowledge. Furthermore, he proposed "tentatively" 4 subgroups: the paranoid, catatonic, hebephrenic, and simple schizophrenia. These wise limitations were, however, forgotten and one became accustomed to accepting the concept of schizophrenia as a unit and the fixity of the subgroups, the certainty of the symptoms and their explanations, and treatment was formulated accordingly.

Some of the fallacies involved become apparent when one considers critically the schizophrenic illnesses from the point of view of physiology, social influences, and cultural significance. There are some physiologic findings which point to the probability of essential physiologic factors in some patients. Progress along this line has been impeded by the investigator's acceptance of the dogma of the concept of *a schizophrenia*, and by his unwillingness to consider the need to formulate a different type of illness which has some aspects in common with the group of schizophrenic illnesses. The social influence on schizophrenic symptoms and reaction-formation has been well stressed by Dr. Fromm-Reichmann's group. The cultural influences have been considered in recent years, but were insufficiently emphasized because one has not studied sufficiently the development of individual schizophrenic illnesses over a period of 30-40 years, nor the changing psychopathology with changes in our own culture.

A brief review of current theories, which are considered basic for proposed therapies, and of the observed psychopathologic phenomena which support these theories, will illustrate the need to be guided by a carefully established psychopathology of the individual patient. When the contributions of Freud and his co-workers influenced that outstanding group of the Burghölzli Clinic, Bleuler, Jung, Abraham, and others, many of whom became leaders in psychoanalysis, outstanding symptoms were those which could be best interpreted by the concept of regression to the oral and anal level of personality development. It is of interest to note that in a hospital with modern dynamic psychotherapy in which interpersonal influences are constantly analyzed and studied, these symptoms of regression of 40 years ago have become rare or have disappeared completely. To this group belong incontinence, smearing, eating of feces, so-called fetal postures, verbigeration and echolalia, refusal to eat, frank catatonic motility disorders, vulgarity in words and in symbolic acts, stereotypies and mannerisms. These changes do not seem to relate to suppression or repression, but rather to lack of activation of oral and anal factors by the changed environmental attitudes and behavior. It is quite

[1] From the Department of Psychiatry, Cornell University Medical College, and The New York Hospital (Payne Whitney Psychiatric Clinic), New York.

[2] This, the foregoing, and the following paper are discussions of Dr. Frieda Fromm-Reichmann's Academic Lecture, read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954, and which appears on page 410 of this issue.

possible, for example, that Adolf Meyer's concept of the catatonic reaction being the response of submission to dominating personalities and situations offers a better explanation than the concept of regression. There are other formulations that are equally attractive and should be considered in psychotherapy.

The concept of narcissism, which is closely linked to Bleuler's autism, deserves critical scrutiny. Dr. Fromm-Reichmann shows such an attitude in her analysis of the schizophrenic loneliness, fear of closeness, and secret hostility. This dynamic formulation is far more helpful in the psychotherapy of patients than that of the narcissistic neurosis of Freud, or of Bleuler's invisible wall between patient and physician. Both of these concepts led to a defeatist, if not nihilistic, psychotherapeutic attitude. In modern hospitals and in private practice most schizophrenic patients reach out for help and do not find protection in autistic withdrawal. Another example is presented by the associative disorders which are said to prevent dynamic interviews. Bleuler's formulation of disturbance of association can no longer be accepted. Psychopathologic investigations have demonstrated that intense anxiety can lead to vagueness of concept formation and incoherence. With the decrease of anxiety these symptoms decrease or disappear. Furthermore, chronic anxiety and acute fear have different significance and effects.

Bleuler's tentative subgroupings deserve careful reevaluation. During a schizophrenic patient's life, significant changes may occur. From a study of World War I schizophrenic patients at the Montrose Veterans Administration Hospital, I feel qualified to state that a considerable number of patients who present paranoid pictures for 2 to 3 years, develop into a simple deterioration. In a relatively small group where delusions were fairly well defined or even systematized, a paranoid picture remained throughout life. Most of the catatonic patients developed into a hebephrenic or simple deterioration. In delusion formation not only depression and elation play a role, as Bleuler postulated, but frequently anxiety and resentment are the dynamically essential emotions.

To base psychotherapy on analysis of symptoms and special psychopathologic reactions as it was proposed by Kraepelin and Jung, and in more recent years by Rosen, is not acceptable from the point of view of dynamic psychotherapy. Dr. Fromm-Reichmann's rejection of the "dichotomy, for therapeutic purposes, between states of manifestly psychotic and, behaviorally speaking, less severe disturbances" deserves to be underscored. It is important that the term "psychosis" which is psychopathologically untenable and which has led to inexact and loose thinking in psychotherapy, be avoided.[8]

The goals of psychotherapy will affect not only the duration of treatment but also the therapeutic procedure. It is proposed that the goal is to enable the patient to develop insight into the genetics and dynamics of his disorder. It is highly questionable whether this is a justified goal for more than a small minority of schizophrenic patients. It is, however, important for the therapist to obtain an insight into the dynamic factors so that he can plan and direct his treatment, help the patient to gain as much understanding as is possible and desirable, and aid the relatives concerned to gain a certain amount of understanding. It is also questionable whether the process of working through, which has been found so essential in the treatment of psychoneuroses, is desirable for schizophrenic patients. It may lead to the persistence or increase of existing anxiety and resentment, and prevent an adjustment to unmodifiable factors, a desirable repression and the integration of a disorganized personality. Most psychiatrists agree that an alleviation of anxiety and of other disturbing emotions may frequently be necessary for progress in a reintegrative psychotherapy. In many patients this is the essential goal that can be obtained. In others one has to help the patient to learn to tolerate emotional reactions and related symptoms, *e.g.*, his inadequacy to obtain the goals which he has set for himself or to accept social factors which cannot be modified or avoided. In some patients one may succeed in establishing self-reliance, with an ability to pursue

[8] See: Diethelm, O. The fallacy of the concept: psychosis. In: Hoch, P. H. and Zubin, J., *Current Problems in Psychiatric Diagnosis;* Grune & Stratton, New York, 1953. Pp. 24-32.

feasible goals, to recognize limitations, and to adjust to the demands of the environment. Although most of us will try to lead or push the patient into the direction of our concept of normality and of an active life in the community, we must keep in mind that this may not be the best result for all patients. There are schizophrenic patients who obtain some degree of contentment or happiness in a place that offers them a possibility to retreat from intolerable or hostile forces and to find asylum in a group of other patients. The best example is presented by the many paranoid schizophrenic or generally inadequate simple schizophrenic patients, who find some kind of companionship and tolerance in a hospital setting or a farm colony of the type which Bleuler and Klaesi developed.

The technical procedure must be adjusted to the highly involved and changing psychopathology. Active direction by the psychiatrist or the less obvious direction by a well-defined goal may be indicated. Alleviation of anxiety and resentment, and reeducation of social and emotional reactions may be obtained directly through psychotherapy, but more often with the additional aid of occupational and recreational activities. The procedure of free association may be helpful and indicated but its use should include an obligation for the psychiatrist to be guided by the appearance of disorganizing factors. Offering interpretations at the wrong time may bring forth overwhelming disturbing emotions and lead to disorganization and dissociative negativism. In some patients insufficient help on the part of the physician will lead to intolerable anxiety and feelings of being rejected, left alone or unaided. The utilization of carefully selected social influences, *e.g.*, through members of the family or friends, through nurses or other patients, may be important therapeutically. The psychotherapy of schizophrenic patients may have to include, directly or through another psychiatrist, aid to important members of the family. There may be opportunities for a fruitful analysis of selected symptoms or of psychopathologic reactions. Such analysis should never lose sight of the total psychopathologic picture and of the desired therapeutic goal. One must remember that a narcissistic perfectionism with its inability to obtain full satisfaction is not related to guilt in the same way as in a depressive reaction in a phase of non-success. Loneliness may be related to unhappiness or to depression and have a different significance. Too much stress on, and curiosity in, symptoms and life reactions may, in the patients, lead to a biased and often interfering attitude. Emphasis on and demand for love, for example, may prevent the patient from recognizing the constructive experience of receiving respect. The physician who is interested in schizophrenic psychopathology may spend too much effort in satisfying his own curiosity and carry out research instead of treatment.

The personal relationship between physician and patient has received much attention from Dr. Fromm-Reichmann. I wish merely to add that a psychiatrist's attitude to schizophrenic patients will change greatly during his life. His peculiar life situation may be felt more in the treatment of schizophrenic patients than in any other group. As a young man he may unconsciously react to the patient as a sexually desirable person or as to a sister or brother; in later life a parental factor may enter into it and still later, new identifications may influence him.

In summarizing the essential aspects of the psychotherapy of schizophrenia I would stress the need to be constantly guided by dynamic psychopathology, with a recognition of physical, environmental, and cultural influences. This concept demands that the psychiatrist observe all psychopathologic phenomena with as unbiased an attitude as possible, with careful attention to the symptoms as such as well as the factors involved. He cannot afford to be guided by theories that cannot be supported by the psychopathologic knowledge of today and must be alert to additional knowledge as presented in his own experience and in medical literature. He must recognize that a schizophrenic patient is not separated from him in an unreachable way and, although insight into the dynamic factors and even in the abnormality of his behavior may be missing, the patient feels in need of some kind of help. The physician-patient relationship should always be an integral part of psychotherapy and persistently scrutinized. The main psycho-

therapeutic tool, the interview, should be adjusted to the patient's need and his changing psychopathology. The social milieu in a hospital with its combination of permissiveness and control is of great therapeutic importance. It offers an opportunity to the patient to reach up to social contact at a level that is acceptable to him. In ambulatory treatment, the same factors should be utilized or created whenever possible. Physical therapies, especially insulin and electroconvulsive therapy may be important aids to psychotherapy, principally for the alleviation of disturbing degrees of anxiety and resentment and their psychopathologic manifestations in schizophrenia. Above all, one must keep in mind the dignity of the individual patient, in the therapeutic interview and in all situations in which the patient may find himself. As a schizophrenic illness is not a well-defined disease entity with the fixity of a progressive condition, therapeutic imagination and plasticity are important so that one can be guided by the facts as they appear in the total picture of the illness. This therapeutic attitude will attempt to uncover essential dynamics as well as foster healthy repressions, search constantly for usable assets, and include the modification of any disturbing physical and social factors.

## III. THE TREATMENT PROCESS[1]

ELVIN SEMRAD, M.D., Boston, Mass.

Dr. Fromm-Reichmann's writings and personal visits have encouraged us working at a state hospital. Her paper, based on experience in quite a different setting, is full of clinical observations with which I agree; although not fully sharing her theoretical implications. We see interpersonal relationships which take in libidinal factors. Anxiety is not the only motive for defense: guilt, disgust, and so forth, may play a significant role. It seems to me that theoretical discussions at this time are largely a question of conviction regarding theoretical inferences based on clinical observations. Therefore, I will only comment on clinical experience which I have found helpful in my work.

Dr. Fromm-Reichmann states that the goal of psychotherapy with schizophrenics is "helping them by a consistent dynamically oriented psychotherapeutic exchange, to gain awareness of the unconscious motivations for and curative insight into the genetics and dynamics of their disorder." In essence I would agree. I would add that, to gain awareness and insight the patient needs a "corrective emotional experience." First, the needs for which the patient seeks satisfaction must be met in the therapeutic relationship so that relief from his anxieties can allow integrative effort on his part, to develop uniformity in the affective expression of his personality. Second, he must be helped to give up reliance on infantile patterns of adaptation, to utilize the more mature latent patterns he has temporarily abandoned, or even to develop new ones which he observes in his "corrective emotional experiences" with his therapist.

Pedagogically, I think there is merit in planning strategy on the basis of the "ego state" in which the patient is met by the therapist. In general, if his ego is in a state of "decompensation," or, withdrawal, or, as I prefer, "strategic retreat" from an "intolerable situation" occasioned by frustration of object need or by a frank loss, the patient will need supportive measures to overcome this "intolerable situation." This must be accomplished not by a covering-up process, but by helping him to face the emotional realities of his adaptive difficulty so that he can return to his optimum ego functioning, which I believe is necessary before he can appreciate himself as a psychosis-vulnerable person. In this regard, the suitable relationship between the therapist and patient is of course the first step in treatment, *i.e.*, the goal is to solve the problems of the doctor-patient relationship. The therapist's own maturity is his main asset, and techniques, as Dr. Fromm-Reichmann points out, must be tailored on an individual basis. Helping the patient solve his relationship problems with other people important in his life becomes the second goal. Return to optimum ego functioning usually coincides with the achievement of these two goals. We refer to this aspect of the work as aiding the "compensation process." Usually at this point the patient is able to leave the hospital.

Definitive psychotherapy in the community, essentially, continued efforts at analysis of ego functioning apropos to vital life issues, allows the patient opportunity to recognize his own contribution to his maladjustment. The therapist can then encourage him to try latent or even new, more mature patterns of adaptation (observed in his therapist and members of his family) and experiment with them on a trial-and-error basis thus achieving skill in social adaptation and successful self-expression. In this way he will maintain emotional security and self-assurance through the affection and response of others. Through respectful but candid interaction in the attitudes of patient and doctor, knotty problems can be settled much more effectively.

We often find it necessary to involve relatives in treatment planning. In our Community Clinic, devoted exclusively to definitive psychotherapy of discharged house cases, we experiment with groups consisting of

[1] This and the 2 preceding papers are discussions of Dr. Frieda Fromm-Reichmann's Academic Lecture, read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954, and which appears on page 410 of this issue.

mothers of schizophrenic patients and offer case work help to members of the patient's family. In a recent study of cases treated by residents, it was found that patients stay in treatment longer if the member of the family involved in the onset of psychosis was also in treatment. Of the patients who continued treatment, 37% have relatives in treatment, as compared with 9% of those who broke treatment. We lay great emphasis on early return to community life and frequently utilize adjunctive treatment to aid the "compensation process." Psychotherapy contacts are preferably maintained during these periods.

That the therapist have and show sincere interest in the patient, is so essential for patients who deny (catatonics) wanting contact with other people. This denial is in our experience, usually the patient's answer to anxiety over lack of affection. Although the patient manifests no ability for interpersonal communication, in his manner he may manifest great readiness for relational contact with the therapist. This has great prognostic significance if one considers the poor prognosis of the patient who uses denial to express his disdain for or fear of further hurt in personal relationships. His need to fortify his denial by projection, distortion, or deeper regression, adds to the therapist's problem. The paranoid patient who bids for contact through projection is but asking the therapist to share his anxiety about his unacceptable inclinations, hostile and libidinal. The patient in a state of delusion and hallucination, bidding for contact by relying on distortion, may be asking the therapist to help him deal with the anxiety incident to his dissatisfactions in life which he feels powerless to change. To understand the purposefulness of the reaction is essential The exaggeration of these patient-response patterns as a result of anxiety, often libidinal, incident to the relationship to the doctor, with psychotic symptoms during the therapy hour, may often result in readmission unless it is understood as part and parcel of the treatment process and an indication of negative transference. A reaction pattern that Dr. Fromm-Reichmann did not mention which often occasions great difficulty to therapists and patients is the clinical state when the patient is symptom-free but reluctant to leave the hospital. This neurasthenic-like state, this tendency of the patient to magnify difficulties which would excuse failure, often seems due to the therapist's expecting reward for his efforts by performance on the part of the patient which the latter interprets as a rejection, *i.e.*, that he no longer is accepted with affection on a personal basis. Any lack of success or the expectation of falling short may be so great a threat to his security as to generate excessive anxiety unless some excuse, such as not showing enthusiasm to return to the community, can be manifested.

In communication with infantile personalities, it is so important not to expect free association when the person is not functioning at his optimum ego level. Under the guise of permissiveness and "waiting for material," the therapist is apt to avoid too long the patient's problems of relational contact. Reliance on associative anamnesis techniques as well as even saying for the patient what he cannot say himself seems indicated. Certainly the need to see a patient through from beginning to end by one therapist and support of that therapist in the process is well pointed out by Dr. Fromm-Reichmann. Apropos of the question of interpretation, I find that in the decompensation period interpretations meaningful to the patient are very difficult to formulate. I found it more effective to make appropriate comments on the interpersonal relationships from which the patient has withdrawn or may be withdrawing at the moment with an effort to keep the patient actively interested in seeking for the emotional truth of his relationships and specifically to consider aspects of them that are full of anxiety. In the compensated period interpretations are very helpful.

I am deeply grateful to Dr. Fromm-Reichmann for bringing us up to date on her thinking and sharing with us her experience.

# THE QUEST FOR A TEST OF CRIMINAL RESPONSIBILITY[1]

MANFRED GUTTMACHER, M.D., Baltimore, Md.

Ever since the formula for criminal responsibility was enunciated in the M'Naghten Rules, in 1843, it has been assailed by leaders in psychiatry. The most important early American critic was that magnificent pioneer, Isaac Ray. In England, Ray's great contemporary, Henry Maudsley, was the chief outspoken critic. In the preface to his *Responsibility in Mental Disease,* Maudsley spoke of

> . . . the scorn and indignation felt by those who observe with impatience the obstinate prejudice with which English judges hold to an absurd dictum, which has long been discredited by medical science.

The opposition of both Ray and Maudsley was based primarily on the failure of the Rules to cover defects of will. Continued dissatisfaction with the M'Naghten formula of responsibility has been voiced by such leaders in American psychiatry as White, Karl Menninger, Overholser, Alexander, and Zilboorg.

The law has for the most part treated the pronouncements of the learned law judges in the M'Naghten decision as sacrosanct. There have, to be sure, been isolated bold souls who have voiced their opposition—such as Judge Sommerville in Alabama and that worthy triumvirate of New Hampshire jurists of the 1860's Judges Doe, Perley, and Ladd. Sir James Fitzjames Stephens, one of the most renowned English criminal law judges, wrote 70 years ago in his *History of the Criminal Law of England,* concerning the M'Naghten Rules,

> The authority of the answers is questionable and it appears to me that when they are carefully considered they leave untouched the most difficult questions connected with the subject, and lay down propositions likely to be misunderstood.

Stephens recognized the fact that there were criminals who were suffering from a defect of will, and, primarily because of it, were incapable of controlling their antisocial impulses. However, he felt that these cases could be adequately covered by the M'Naghten Rules, since, in his opinion, it could be held that there can be no significant defect of will without a deficiency of intellectual powers. Jerome Hall, one of the most doughty of the modern legal defenders of the M'Naghten Rules, takes a similar position in rejecting the concept of the irresistible impulse.

Today there is reason to believe that the discontent of the leaders of the law with the M'Naghten Rules is becoming quite general. No longer are there mere isolated bursts of rifle fire, but there are salvos of big guns being directed at the Rules on both sides of the Atlantic.

Chief Judge Biggs, of United States Court of Appeals for the Third Circuit, wrote recently in his dissent in *Smith v. Baldi,*

> The Law, when it requires the psychiatrist to state whether in his opinion the accused is capable of knowing right from wrong, compels the psychiatrist to test guilt or innocence by a concept which has almost no recognizable reality. . . . The Court in compelling the answer to this question was adhering to the law of Pennsylvania of over a hundred years' standing. It need not always be so. Changes can be effected and reason can be brought to the law of criminal insanity. The rule of M'Naghten's case was created by decision. Perhaps it is not too much to think that it may be altered by the same means. If not, then legislation must prevail.

The objections to retaining the M'Naghten Rules in their present form has seldom been put more effectively than by Mr. Justice Frankfurter, when he testified recently before the Royal British Commission on Capital Punishment:

> . . . The M'Naghten Rules were rules which the Judges, in response to questions by the House of Lords, formulated in the light of the then existing psychological knowledge . . . I do not see why the rules of law should be arrested at the state of psychological knowledge of the time when they were formulated. . . . If you find rules that are, broadly speaking, discredited by those who have to administer them, which is, I think, the real situation, certainly with us—they are honoured in the breach and not in the observance—then I think the law serves its best interests by trying to be more honest about it . . . I am a great believer in being as candid as possible about my institutions. They are in large measure abandoned in practice, and therefore I think the M'Naghten Rules are in large measure shams. That is a strong word, but I think the M'Naghten Rules are very difficult for con-

[1] Read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954.

scientious people and not difficult enough for people who say "We'll just juggle them" . . . I dare to believe that we ought not to rest content with the difficulty of finding an improvement in the M'Naghten Rules. . . .

Perhaps most of you are acquainted with the fact that the American Law Institute, aided by a large grant from the Rockefeller Foundation, is now engaged in drafting a Model Code of Criminal Law. This task, one of great magnitude and one of very great importance, is being directed by Professor Herbert Wechsler, of the Law School of Columbia University. The Institute has appointed an Advisory Committee composed of leading judges and lawyers and representatives from some of the behavioral sciences, sociology, anthropology, criminology, and psychiatry. Psychiatry has, among these ancillary disciplines, one of the strongest groups of representatives, as far as numbers go, composed of Winifred Overholser, Lawrence Freedman, and the author of this paper.

Professor Wechsler assigned to me the very difficult task of writing a memorandum on the psychiatric aspects of the rules of criminal responsibility. This matter will be brought before the Advisory Committee in the near future for their recommendations to the Institute. It would be of inestimable value to me and to Drs. Overholser and Freedman, if following the discussion of this paper, it were possible to obtain from this group, which is certainly representative of the psychiatrists of Canada and the United States most interested and experienced in legal psychiatry, a definite expression of opinion in regard to the rules of responsibility.

Perhaps it would best serve our purposes if we should at this time consider the formulas that have been suggested at various times and then analyze them.

First, there are those who would do away entirely with the plea of insanity. Most of the proponents of this plan suggest, as a corollary, that the criminal trial be restricted to the inquiry whether the accused actually committed the act charged. If it is so found, he is then referred to a dispositional tribunal that assigns him, after thorough study, to the proper authority, whether it be a probation department, a penal institution, or a mental hospital.

Second, there are the M'Naghten Rules. These are so familiar to us all that there seems little point in outlining them. Suffice it to say that the really significant element in them is the dictum that for a man to be exculpated because of mental disease he must be so affected "as not to know the nature and quality of the act he was doing, or, if he did know it, that he did not know what he was doing was wrong."

Third, there are those who would add the irresistible impulse rule to the knowledge of right and wrong of the M'Naghten Rules. This is essentially the law in a third of the states and in courts of Federal jurisdiction.

A fourth proposal is that criminal irresponsibility because of mental disorder be equated with civil committability to a mental hospital.

Fifth, there is the New Hampshire Rule which rejects all legal tests that have been devised and holds that the question of irresponsibility is one of fact for the jury. Under this law, if the defendant has a mental disease and if the criminal act is the product of it, he is found not guilty by reason of insanity. The fundamental principle behind this is that criminal responsibility requires a guilty intent, or *mens rea*, as well as a prohibited act.

Let us briefly consider each of these in turn. The first proposal is that the insanity plea be done away with. Many thoughtful psychiatrists who have had court experience are so impressed with the difficulties in rendering conscientious service to the courts in the determination of criminal responsibility under existing rules, and are so overwhelmed by the challenge of devising a substitute formula, satisfactory both to law and medicine, that they have become nihilists. Giving up the insanity plea would also make largely unnecessary the sweeping reforms in the methods of presentation of expert testimony that are as greatly needed as are changes in the tests of responsibility themselves. However, such problems are beyond the scope of this paper and cannot even be touched upon. Two states, Louisiana and Washington, have enacted laws doing away with the insanity plea and both have been found unconstitutional by their Appellate Courts. As far as I can learn, legal scholars are doubtful that such an enactment could be drawn that would be found constitutional.

Moreover, it seems to me that the principle of exculpating the mentally diseased is essentially a just and humane one. The enormous difficulties of the problem should not force us to take an essentially unsound position.

The second proposition is the one that considers the M'Naghten Rules sacrosanct and inviolate. Many lawyers are of the opinion that these Rules have served us well for more than a century and that it would be dangerous to tinker with them. There is certainly by now abundant evidence that the foundations of our society will not be shaken by amending or even by wholly rejecting the Rules. There are no data to suggest that the incidence of homicide is unusually high in those jurisdictions that accept the irresistible impulse as an addition to the Rules; nor is such the case in Scotland, where in large measure the M'Naghten Rules are disregarded; nor in New Hampshire, where they were totally rejected soon after they were pronounced. I can see no danger in enlarging the category of the criminally irresponsible as long as we hold firmly to the view that every individual is socially accountable for his acts. The truth of the matter is that the finding not guilty by reason of insanity has not resulted in the premature release of offenders into the community. Dr. William Alanson White made a study many years ago showing that, on the average, perpetrators of homicide committed to institutions for the insane spent more time in confinement than those sentenced to penal institutions.

Heretofore, all polls of psychiatric opinion have shown general dissatisfaction with the M'Naghten Rules. Three years ago, when I was chairman of the Legal Aspects of Psychiatry Committee of this Association, I sent out a questionnaire on this subject. Only 12% of the members who replied expressed satisfaction with the M'Naghten Rules.

A third proposal is that the knowledge of right and wrong rule be supplemented by the concept of the irresistible impulse. To this many objections have been raised. There are among the psychiatrists those who maintain that no impulse is irresistible. Frederic Wertham declares:

> The medico-legal theory of the irresistible impulse is advocated only by lawyers and by psychiatrists who are scientifically not sufficiently oriented.

There are others who hold that it is impossible to determine the irresistibility of an impulse, and still others who make use of a *reductio ad absurdum* by maintaining that the mere fact that an impulse was not resisted shows that it was irresistible. In a poll taken of the members of the Group for the Advancement of Psychiatry more than 90% were of the opinion that the existence of the irresistible impulse is a psychiatrically valid concept.

As I mentioned earlier, Professor Hall of Indiana University and others have maintained that it is unnecessary to add this element to the tests of responsibility because everyone laboring under an irresistible impulse has a corresponding inability to distinguish right and wrong and to realize the nature and consequences of his acts. This position seems to me psychiatrically unsound. While it is true that we no longer subscribe to the doctrines of faculty psychology with its recognition of discrete compartments of the mind, surely we have all seen individuals who are totally incapable of controlling asocial impulses and yet whose cognitive functions are disturbed to a far lesser degree and who must necessarily be considered responsible, unless the M'Naghten criteria are very broadly and very capriciously interpreted.

If the burden of proof of insanity were shifted from the defendant to the prosecution, which is already the rule in 19 states and in the Federal Courts, the M'Naghten Rules, supported by the irresistible impulse, might prove adequate from a practical point of view. Offenders suffering from even an incipient schizophrenia should, in nearly every instance, show sufficient pathology to create a doubt as to their responsibility. Although I recognize the fact that some psychiatrists, as well as some lawyers, may feel that it is necessary to furnish jurors with a well-marked, even though not easily read, yardstick, I rebel at the idea of attempting to define with exactness so vagarious and highly individualistic an entity as insanity.

The fourth suggestion is that of equating criminal irresponsibility with civil committability to an institution for mental disorders. This seems to me of doubtful value; the questions involved are historically and socially dissimilar and little is to be gained by equating them. Committability rests upon 3

criteria stated admirably by Chief Justice Shaw of the Massachusetts Supreme Court in the Oakes case in 1845:

> The question must then arise in each particular case, whether a patient's own safety, or that of others, requires that he should be restrained for a certain time, and whether restraint is necessary for his restoration, or will be conducive thereto. The restraint can continue as long as the necessity continues. This is the limitation, and the proper limitation.

Despite the fact that this enlightened decision was written more than a century ago, there exist today many courts that are still unwilling to order enforced institutionalization on the ground that the patient has a mental disorder that will improve with hospital treatment, when the patient lacks the insight necessary to realize this. So that equating irresponsibility with committability does not even insure us uniformity, at present.

The concept of responsibility involves several basic elements. By inflicting penal sanctions, society temporarily or permanently incapacitates the offender and aims to prevent repetition of his offense. It further hopes to deter others from perpetrating similar antisocial acts. There is also inherent in the punishment of the criminal society's need for retaliation.

Whether an individual should be forced into a civil mental hospital is fundamentally a public health problem and only secondarily a legal one. The present trend is toward recognizing this and nearly every year some state forgoes the legal technicalities previously required for enforced hospitalization of the insane.

But whether an individual who has done a wrong against another should be punished or treated as a sick person is not primarily a medical problem. It is primarily a legal problem and only secondarily a medical one. As the lawyer, Edward De Grazia, put it in a recent book review, since legal insanity "must always be at bottom a matter of the custom and the opinion of the community, much reason exists for community (judge-jury) judgments of mental responsibility."

Now let us consider the fifth and final plan, that of making insanity a fact in the criminal trial to be adjudged by the court without any specific tests. This essentially is the New Hampshire rule.

The history of the New Hampshire Rule is of sufficient interest and importance to detain us for a moment. Happily, there has been preserved in the medical library at Butler Hospital, a remarkable correspondence between Dr. Ray and Judge Doe. Much of it appeared in a recent issue of the *Yale Law Journal,* in a truly fascinating article by one of our members, Dr. Louis Reik. Reik points out that Dr. Ray advocated 2 important principles:

> The external manifestations of mental disease are so inconstant and uncertain that no legal definition or test of universal application is possible(2). There is a form of mental illness in which behavior is so disordered and can be so detrimental either to the patient or to others, or to both, that, in spite of seemingly intact intellectual ability and the absence of delusion, the patient seems to be the victim of emotional or "moral" forces beyond his control.

Judge Doe's contention was that the common law holds that any contract that is the product of a mental disease cannot be considered a valid contract nor can an antisocial act of such origin be held to be a crime. He also contended that the common law does not distinguish "only a certain kind or degree of insanity as having any legal consequences." He held further that

> Whether, in any particular case there is a mental disease, and, if there is, whether a certain transaction is a product of that disease,—are questions of fact for the jury and not of law for the court.

As a consequence, according to his view, the court has no right to hold jurors to any specific test of insanity.

In 1869 Judge Doe wrote to Dr. Ray:

> If they [the courts] can be brought to leave the questions of medical science to the jury as questions of fact [which they now do in regard to all other scientific questions], the long controversy between your profession and mine will be brought to an end, and you will be at liberty to convince the jury on subjects concerning which the courts for a long time refused to be convinced, assuming as they have that upon the subject of insanity the doctrine of medical science and the doctrine of law were antagonistic. To remove this antagonism seems to me an immense gain. My faith is very strong that it can be done within one hundred years on no other basis than the principle I have undertaken to establish.

Perhaps it behooves us to labor hard during the next 15 years, so that we do not let the good judge down.

The Royal Commission on Capital Punishment has made a very detailed and scholarly

inquiry, both in England and in this country, into the tests of criminal responsibility. This Commission of 12, had only one psychiatric member, Dr. Eliot Slater, while it had a group of distinguished law members, including a Cambridge and an Edinburgh University law professor and a former Home Secretary. After 4 years' study, from 1949 to 1953, they concluded

> . . . that the test of responsibility as laid down by the M'Naghten Rules is so defective that the law on the subject ought to be changed [and that] a preferable amendment of the law, would be to abrogate the Rules and leave the jury to determine whether at the time of the act the accused was suffering from a disease of the mind (or mental deficiency) to such a degree that he ought not to be held responsible.

I must admit that I derived great satisfaction from this conclusion because, a few days before it reached me, I had drafted my preliminary memorandum to Professor Wechsler with similar conclusions. In it I had said:

> It seems to me that all that should be expected of the psychiatrist is the following:
>
> 1. A statement as to whether the defendant is suffering from a definite and generally recognized mental disorder and why and how this conclusion was reached.
>
> 2. If it has been asserted that the defendant suffered from a mental disorder, its name and its chief characteristics and symptoms, with particular emphasis on its effect on an individual's judgment, social behavior, and self-control, should be given.
>
> 3. There should then follow a statement of the way and degree in which the malady has affected the particular defendant's behavior, especially in regard to his judgment, social behavior, and self-control.
>
> 4. He should then be asked whether the alleged criminal act was, in his opinion, a product of the mental disorder.

In making use of such a formula as this, one might wonder whether there need be laid down a legal definition of mental disorder or mental disease. Judge Doe also gave us an answer to this. He wrote, in 1866:

> The law does not define disease,—disease is so simple an expression that the law need go no further. What is a diseased condition of mind is to be settled by science and not by law,—disease is wholly within the realm of natural law or the law of nature. The municipal, civil law established by men for human government does not declare what is disease of the mind any more than it declares what is disease of the lungs or liver.

One must be proud of this backwoods American jurist, proud of his courage and his sagacity; particularly when one compares his grasp of mental disease with that of the Lord Chancellor of England, his contemporary, who deplored the introduction into criminal law of medical opinions and medical theories proceeding "upon the vicious principle of considering insanity as a disease."

Since sending my preliminary memorandum to Professor Wechsler, I have come to the conclusion that if such a policy as we have suggested is enacted, it would be preferable to use the term mental disease rather than mental disorder. Mental disorder is too tenuous a concept to write into law.

No doubt, there are some who fear that a jury will find certain psychopaths or character neurotics irresponsible, under the rules which I have suggested, even though the rules state that the accused must be suffering from a mental disease. This does not create in me any consternation. Some of the more severe cases of character disorder are in truth seriously sick people. The mere fact that our mental hospitals have not been designed in the past to accommodate and treat these cases is, in my opinion, no reason for forming a law specifically to exclude them. It seems probable that were our hospitals no longer to place their emphasis on their rejection, but rather on their admission, new strides might be made in understanding and treating behavior disorders.

Certainly there is nothing radical nor original about proposition number five. Doing away with test phrases to assay responsibility has the dignity and authority of time and of important names in both law and medicine. It was advocated a third of a century ago by a distinguished committee of the American Institute of Criminal Law and Criminology under the chairmanship of Professor Edwin Keedy of the Law School of the University of Pennsylvania, with such leaders in psychiatry as Morton Prince, William White, and Adolf Meyer as members.

Perhaps I should not have expressed my preference, before asking for yours. But I believe that I am sufficiently acquainted with the members of this organization to conclude that this will surely not seduce you into advocating the fifth proposition—possibly my

concern should be whether it might make you rather an advocate of one of the others.[2]

## DISCUSSION

Val B. Scatterfield, M. D., St. Louis, Mo.—Again we have enjoyed one of Dr. Guttmacher's clear, logical presentations. In this essay, as in his recent book, Dr. Guttmacher shows a keen presentiment that we are on the threshold of a new era in the management of the dyssocial individual. Society is still contaminated by the mystical notions of inevitable retribution and social punishment that provided the early Greek tragedies with fatalistic themes.

The history of crime and punishment is a sorry story. What could be more pathetic than the prisoner in the "press yard" withstanding torture and hoping that death would come before he confessed—perhaps falsely—thereby losing all his property through confiscation, and impoverishing his family. The loved one buried at the crossroads, transfixed with a stake, was not the less loved because of his suicide. The disemboweled watching his genitals burned before him was rendered no less loyal to his own convictions. The children sentenced to death for small thefts and being reprieved, confined on the hulk Euryalus in the Thames waiting for deportation, were no less children than children are today. All of the talionic notions behind these punishments must be removed from the public mind before we have public opinion that will accept modern theories of irresponsibility and correction.

To be able to pass through and beyond the M'Naghten rules we must find about us similar social forces that brought about the adoption of the Rules. How strange it is that everyone speaks of M'Naghten and no one of Peel? Yet the time was insistent for reform. The Napoleonic wars had ended, the soldiers were home and restless, many of them socially ill. The industrial revolution was carrying away many long-established means of livelihood. The Enclosure Acts—Pitt's acts confining the trade unions—the unwise Corn Laws, starvation, and unemployment brought about, among many other social convulsions, the "Peterloo" massacre—the abolition of the "rotten boroughs" and the overthrow of the Test Acts. Among other things, the M'Naghten rules sprang into being from the demand for social justice. There is reason to believe that the attempted assassination of Sir Robert Peel at Whitehall was largely political and attributable to Scottish resentment against the modification of the anti-Catholic laws. The mad Scot, M'Naghten, and Sir Robert Peel's secretary, Mr. Drummond, were small figures in a huge social conflict. Along with the M'Naghten Rules great reforms were effected, and largely at the hands of this same Sir Robert Peel. We should be aware of them because many were of moment in the reform of criminal laws and together with the whole social scene were part of the inspiration that activated our own Dr. Isaac Ray. Sir Robert Peel established civilian police as we know them and the English still call them "Bobbies" and "Peelers" in his memory. Peel was instrumental in abolishing the death penalty for a hundred different crimes. The ideas and work of Bentham, James Mackintosh, and Romilly would have gone for naught in the reformation of the criminal law if Peel had not worked to bring about the enabling legislation. We need another Sir Robert Peel at this time.

We find ourselves suddenly inspired by the ideas of those dead a hundred or even thousands of years ago. One aspect of the consideration of Dr. Guttmacher's propositions reminds me of the second of Cicero's 3 virtues. In *De Officiis*, Cicero writes, "The second virtue lies in the restraining of those troubled movements of mind which the Greeks call 'passions,' and in the making of the 'impulses' obedient to the reason."

One of the stumbling blocks of criminal justice "reform" is the concept "exculpation" which is derived from "morality considerations." There is little evidence to suggest that any legal mumbo jumbo can exculpate man in the mind of God or himself. This depends rather on some spiritual process. Is there any truth in the notion that by administering punishment after a legalistic ritual we seek to "exculpate" ourselves? A conscientious study of Dr. Guttmacher's propositions will permit a quick rejection of proposition 2 and 3 because they are based upon archaic moralistic considerations that have been used by the community to save from unjust fate those who were considered to "know not what they do."

The general principle behind this "mercy" has resulted in the mentally ill, the juvenile, and the first offender being released from social custody without being afforded that corrective help society owes to the dyssocial.

Proposition 5 provides all the elements necessary for the administration of fundamental justice as our culture envisions justice. Juries should be given the evidence and covering opinions and be permitted through reflection to determine for themselves the facts and the place and quality of the offense within the frame of our social life and, by their decision, to assert the right of society to assume the responsibility for the reclamation of the dyssocial individual, sane or insane. Public education will be required to alert potential jurors to modern psychiatric thinking.

Oliver Wendell Holmes once said that justice required that each man get his desserts and have his "needs" fulfilled. The crucial word in the consideration of the offender should not be "guilt" but "needs." Much of our difficulty with the social fear and rejection of the plea is the anxiety that this finding of "insane" is synonymous with the word "free" and that the individual will thus be thrown back on the community as the average juvenile and "first offender" often is, without sufficient thought about what the offender requires in the way of social treatment. The proper disposition of the "responsible" and the "irresponsible" will be the very heart of the future social proceedings of which one part is now called, "The Administration of the Criminal Law."

[2] The results of the balloting by those who heard the paper read were: for proposal 1, 3%; for proposal 2, 2%; for proposal 3, 3%; for proposal 4, 7%; for proposal 5, 85%.

# PROGNOSTIC STUDIES IN MENTAL DISORDER[1]

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More accurate prognosis of mental disorder has been the goal of much research. In addition to studies by psychiatrists, there have been investigations by psychologists, sociologists, social workers, and biometricians, either independently or in collaboration. Windle(32) in 1952 made a comprehensive survey of the use of standard psychological tests in psychopathological prognosis after he had found previous reviews of this subject were not sufficiently critical. A need remains for a critical survey of prognostic research examining factors other than psychological test performance. This article will attempt to evaluate critically the published research from the viewpoint of the methodology rather than substantive content, since the writer holds the latter is not a fruitful endeavor. Reference to previous reviews may clarify this point.

## APPROACH OF OTHER REVIEWS

In what purported to be a critical survey of prognostic criteria in schizophrenia, Chase and Silverman(7) reported the prognostic value which had been attributed to commonly investigated factors. When these reviewers found agreement concerning the association between any factor and outcome, that factor was considered to be important in prognosis. When the literature evidenced disagreement concerning the association between any factor and outcome, that factor was dismissed as not contributing greatly in prognosis. On the basis of consensus found in the literature, Chase and Silverman concluded that certain factors had prognostic value.

This process of arriving at the truth in prognosis may be misleading when it has not evaluated the degree of comparability of studies. These may have differed with respect to definitions of variables, criteria for classifying outcome, the time at which outcome was classified, and the point in the patient's career at which the prediction was made. A factor, for example, which may be useful in predicting recovery at the time of admission, may not necessarily have the same value in predicting outcome at the time of release.

Even when 2 studies are comparable, the findings of one or both may be invalid because of deficiencies in design or because application to the data of statistical tests reveals a reported finding to be unsupported. Several invalid studies similarly biased on the meaning of a given factor would not bestow any prognostic quality on that factor. Nor would invalid studies detract from the results of valid studies because of disagreement on the prognostic value of a particular factor.

The device of combining studies has appeared in other reviews. Stalker(27) combined the data of 16 prognostic studies made between 1921 and 1929. Hunt, *et al.*(14), combined figures from 11 studies, stating that:

> Although there are considerable discrepancies between individual reports, the averages arrived at by combining the figures are probably not far from the true rates in a series of unrelated cases (14, p. 414).

This indicates a misinterpretation of the meaning of randomization, which is apparently assumed to obtain because of the lack of conscious bias of the various authors whose data were used.

Malamud and Render(20) in 1939 criticized prognostic studies on the following grounds: First, the fact that frequently samples of only 25-30 cases were used. Mere size, however, does not assure representativeness. Valid generalizations may be made from relatively small samples which have been randomly selected to represent the parent population and the situation being studied.

Second, short duration of the follow-up study: "Patients who have been followed for only one or two years after discharge are not suitable for conclusions concerning ulti-

[1] This paper is based on work done during the tenure (July 1952 to September 1953) of the author's Research Fellowship, National Institute of Mental Health, U.S.P.H.S., and is part of a larger project supported by that agency.

mate outcome of the disease." The truth of this as it concerns ultimate outcome may be granted. However, a valid prognostic study may still claim to predict outcome for any period in which an investigator might be interested.

Third, the lack of uniformity in evaluating the original symptomatology as compared with the condition at the time of the follow-up study: Malamud and Render recommended follow-up evaluation by the original observers. Without discussing the merits of using symptomatology as an index of post-hospital adjustment, the evaluation of the patient's status preferably should not be undertaken by the observer responsible for diagnosis and perhaps treatment. The need to preclude the introduction of bias in his observations requires the rating of the effect of a treatment program by an observer who has no emotional investment in the diagnosis and treatment. The difficulty of finding a detached observer frequently confronts therapists with the problem of evaluating their own work, although this is not the most desirable procedure.

Fourth, failure to indicate the criteria utilized in making the diagnosis: This is a valid criticism and should, as has been indicated, be generalized to include criteria utilized in all factors studied.

Fifth, the inadequacy of the follow-up examination: If presence or absence of symptoms is to be the criterion, an examination by a trained observer is preferable to correspondence with relatives, reports from neighbors, and other sources considered unreliable by Malamud and Render.

Sixth, failure to control the type of treatment during hospitalization, since patients who have received intensive treatment will "naturally" show better results than those who were simply observed in the hospital: The authors imply type of treatment should be held constant, although it might well be treated as another variable whose association with outcome should be investigated, and not accepted as natural.

Seventh, an evaluation of the results should take into consideration the effect of preconceived notions and the interests of the physician in a particular form of treatment. There can be no quarrel with this, although the authors saw no relation between this statement and their third point.

Malamud and Render did not apply the above general criticisms of research to specific studies. Reviews of the type made by Chase and Silverman(7), on the other hand, present the findings of specific studies without evaluation of the methodology. With the conviction that scientific investigators welcome criticism, this writer will examine the research methodology of individual studies, most of which have been published in some of the more widely circulated journals. Reference will be made to data only as it clarifies the discussion of methodology. The studies reviewed will be grouped according to the type of research design apparently utilized by the investigator. Although other groupings are possible, this classification seems most appropriate to this review.

### EX POST FACTO EXPERIMENTAL DESIGN

This was the most common research design encountered. Chapin(6, p. 10) has referred to it as one

> in which some present effect is traced backward to an assumed causal complex of factors or forces operating at a prior date, using for this purpose such records as are available, since no new measures of the past can be made in the present.

When antecedent factors which have been previously hypothesized to be relevant in prognosis are found to differ significantly in each group, they are assumed to be associated with the outcome. In prognostic studies the patients are sorted into two or more groups according to some criterion applied subsequent to illness, *i.e.*, presence or absence of original symptomatology, on leave or hospitalized, etc. While each researcher has a right to formulate his own hypothesis the sampling should be faultless and certain rules of logic should be followed. The outcome categories must, of course, be mutually exclusive, exhaustive, and appropriate. Some test of significance must be applied to observed differences to determine whether they are simply chance variations. These *ex post facto* studies may be further classified according to the time at which the prediction would be made. Some attempt to find factors that will permit a prediction of outcome at the time of the patient's admission;

others, at the time of release. The former usually study samples of admissions; the latter, samples of released patients. It must be realized that conclusions based on the latter type could not legitimately be applied to a group of new admissions but at best would apply only to patients ready for release. Conclusions, which must be based on the data collected, apply only to the population represented by the sample.

### STUDIES USING PATIENTS SELECTED FROM ADMISSIONS

In an early study Bond(3) classified 200 patients according to traits indicative of social, normal, abnormal, or seclusive prepsychotic personality. He then compared the extreme personality types, social and seclusive, with respect to outcome. Finding more cases with desirable outcome in the group with a social prepsychotic personality, he concluded that that personality type was a favorable prognostic factor. This conclusion is weak on several counts. Before the 200 cases were selected, an unknown number had been discarded, because of lack of information on personality. The possible bias was not evaluated. Choosing the extremes of the personality continuum for investigation is a good practice, but Bond dropped from these groups all cases other than those diagnosed as manic-depressive or schizophrenic psychoses. This could be supported, except that Bond failed to qualify his findings as limited to these cases, but generalized concerning the association of social and seclusive prodromal personality types with outcome. Further, no statistical tests of significance were applied to the observed differences.

Strecker and Willey(29) classified 186 consecutive admissions into 2 groups, recovered and not recovered. The criterion was not further defined, nor was the date given when outcome was evaluated. The predictive value of certain factors was investigated by comparing the outcome groups with respect to the frequency of each factor. The conversion of frequencies into percentages obscured the smallness of the former and enabled the investigators to report that one group showed a high proportion of Jews (N=4). No test of significance was used. The conclusions(27, p. 38) were embellished with prognostic dicta not tested in the research:

> If psychosis is an evolution of such peculiarities (*i.e.* "constitutional" rather than "reactive" seclusiveness) and no deterioration . . . is implied, the outlook is not necessarily hopeless.

There had been no attempt to make this differentiation in classifying the factor of personality although the authors believed it to be an important distinction.

Four years later Strecker(28) reported a follow-up study of 25 of the group of 38 "recovered" patients, a study made to "see if they [the prognostically favorable factors] had stood the test of time." Finding that these patients had continued their adjustment, he claimed validation for the previous study's conclusions regarding predictive factors. The reviewer's skepticism is supported by the unexplained loss of 13 cases. The follow-up hardly constituted a test of the validity of the prognostic indicators. Reexamination of the patients, assuming the missing patients had also maintained remission, might have settled any question of the stability of the criterion but did not answer the pertinent question whether the earlier research had derived valid prognostic items. The acceptability of the definition of a criterion does not *ipso facto* render acceptable the prediction instrument.

Investigating the association between onset and outcome in schizophrenia, Sullivan (30) began with 155 consecutive admissions, eliminating 55 because of defective information, questionable diagnosis, and schizophrenia with mental deficiency. The bias caused by the loss of cases due to defective information is not known. A higher proportion of the patients with acute onset recovered than of those with insidious onset. The importance of the lost cases is emphasized by the fact that the discriminatory power of insidious onset was based on finding 7 improved cases and 5 unimproved. Recovery was defined in terms of cessation of schizophrenic processes, resumption of social living with gradual expansion of life interests. Sullivan did not give the time period during which these indices must have been in evidence in order to classify the patient properly.

Sullivan(30, p. 115) concluded:

> Disregarding all the factual material which can be solicited in psychopathological study of individual patients, one may give a heavy favorable weighting to the dramatic outcropping of the psychosis.

This license to ignore all other elements in a particular case is not justified by Sullivan's study which made no effort to control other factors which might have been related to outcome. This requires the use of some method of matching or measuring the criterion groups on contingent factors, or conditions which may operate jointly with onset if a particular effect, either recovery or nonrecovery, is to follow. Lacking these controls, Sullivan's study is hardly definitive.

Malamud and Render(20) studied 344 consecutive schizophrenic admissions of whom 35 inaccessible cases were dropped. To control length of follow-up period, 3 groupings according to time released were made. These groupings were subdivided further according to 6 categories of outcome. With the exception of the death category, the outcome categories were poorly defined. Although the authors probably believed they were dealing with a large sample of 344, since they had criticized samples of 25-30 cases; it should be noted that the completely recovered group contained only 25 cases. Patients who had died were not considered, an important omission, since the deceased may have demonstrated remission for some years before death. The criterion, however, necessitates an evaluation of symptomatology, despite the fact that the patient may have led a productive posthospital life.

Prognostic significance was attached to those factors found in differing proportions within the completely recovered and unimproved groups. No statistical test of significance was used to verify impressions. Cyclothymic personality was considered favorable, for example, because it appeared proportionately more frequently in the completely recovered group; schizoid personality, unfavorable, since it appeared more frequently in the unimproved group. Table I is derived from data given by Malamud and Render (20, p. 1044).

Using the method of chi-square to test these distributions for independence we find that the observed differences can be explained as chance variations ($X^2=3.07$; d.f., 3; probability, $<.5 >.3$). This illustrates the undependability of mere inspection as a means of determining the significance of observed differences.

TABLE 1

PREMORBID PERSONALITY AND OUTCOME OF PATIENTS FOLLOWED 5 YEARS

| Personality type | Completely recovered | Unimproved |
|---|---|---|
| Normal | 7 | 34 |
| Cyclothymic | 2 | 3 |
| Schizoid | 7 | 39 |
| Miscellaneous | 9 | 26 |
| Total | 25 | 102 |

Taylor and Von Salzen(31) studied 1,100 first admissions after excluding an unknown number who left the hospital on other than parole status. They then described the group with respect to certain factors. Another unmatched and undescribed group of 74 insulin treated patients was compared with the group of 1,100 patients. Finding that 25, or 62%, of the 40 insulin patients who had been psychotic for 2 years or less prior to admission had been paroled, whereas only 342, or 54%, of the same group of the 638 noninsulin treatments had been paroled, the authors concluded insulin treatment made for a favorable prognosis. The authors admitted that the difference was "perhaps small enough to be accounted for on the basis of the more personalized care on the insulin wards." This illustrates the importance of controlling factors which might obscure the relationship between some given treatment and outcome. Application of the chi-square test to Taylor and Von Salzen's data supports an alternative hypothesis, namely, that the difference is small enough to be accounted for on the basis of chance ($X^2=1.48$; d.f., 1; probability $<.30 >.20$).

Stalker(27) classified outcome of a series of schizophrenic first admissions as either complete remission, social remission, improved at home, remitted and relapsed, unimproved. These categories were described with varying degrees of clarity. The method of analysis is not discussed; apparently, the outcome groups were compared with respect to the relative frequency of occurrence of certain factors. Definitions of these factors

were obscure. For example, the author undertook the estimation of the relative proportions of constitutional and environmental factors. Work, social, and sexual adjustment were rated as inadequate or adequate, but the definitions offered would not facilitate replication of the study.

Dunham and Meltzer (8, p 123) attempted to predict length of hospitalization claiming such a scale would be:

> useful in selecting those patients who could respond more adequately to present therapeutic procedures . . . for assigning patients to the acute or chronic sources . . . furnish psychiatrists with an objective instrument for prognosis.

The criterion was variation in the length of hospitalization. The mean length of hospitalization of the total group was calculated. Information on a number of personal history factors, e.g., age, maturity, marital status, educational level, etc, was obtained. Each of these factors was subdivided. The mean length of hospitalization was calculated for each of these subgroups. The significance of the difference between the mean for each subgroup and the means for the total number of patients was evaluated using the method of critical ratios. A subfactor was weighted in accordance with the magnitude of the critical ratio. Each patient was scored, on the personal history factors, using the weights derived. The total weighted score of each patient was correlated with the criterion. Simply stated, an effort was made to see if certain factors in a patient's case would be useful in improving the prediction of length of hospitalization one could always make by predicting the patient would be hospitalized for the time found to have been the mean for all patients. Results were admittedly inconclusive.

No consideration was given any contingency factors, particularly treatment received after the time when the prediction would have been made. Until the effect of treatment is known or controlled, one might question the basis for recommending treatment of patients who ordinarily show the best prognosis in terms of length of hospitalization and withholding treatment of other patients.

Treatment, even when it does not reduce the length of hospitalization, may be beneficial. The patient who may not be able to reach a level of adjustment which warrants release may still benefit from treatment which facilitates his functioning better in the hospital. Dunham and Meltzer's approach assumes that effectiveness of treatment can be evaluated without reference to the therapeutic goal for each patient.

Their research seems to have been based on a false premise, namely that treatment procedures are uniformly available to each patient. It follows that patients, whose longer hospitalization indicates a poor response to treatment, should have been consigned at admission to the custodial wards. The next step is to identify these patients by certain personal history factors so that new patients resembling the failures can be immediately shunted to chronic wards.

The basic premise in this logic is faulty because the quality and quantity of treatment available may be influenced by factors not considered by Dunham and Meltzer. For example, the situation of a particular patient may have a poignancy which inspires the staff to greater therapeutic effort. Family interest is another factor which may produce the same effect. A predictive instrument which did not evaluate these factors might handicap the patient by pointing to his admission to a chronic ward.

Rupp and Fletcher (26) made a follow-up study of a series of 641 schizophrenic admissions, which were grouped into 4 outcome categories: much improved, improved, unimproved, and dead. Patients hospitalized at the time of follow-up were considered "unimproved." Not infrequently, however, patients are still hospitalized long after psychiatric improvement. Prolonged hospitalization may be due to the relative's loss of interest or death, and the hospital's inability to find a suitable placement. An improved working patient may remain hospitalized for extended periods when a cautious administrative policy insists on fulfilment of the usual legal provisions of parole. A patient's presence in a hospital is not evidence of the presence of symptoms.

The association between the criterion and certain factors was estimated by inspection of the percentage frequencies with which any sub-class of a variable was represented in

outcome groups. Conclusions based on visual inspection of observed differences may be misleading. Results are placed in question also by the loss of 122 cases, and the follow-up by letters only of another 125 cases.

Rennie(24) sampled 208 cases of manic-depressive psychosis, after discarding 99 cases because of unavailability of follow-up information or lack of a clear-cut diagnosis. We do not know how many were dropped for the former reason, an important point which affects the sample's representativeness. Patients were placed in various outcome categories (recurrent, alternating, lasting recovery), and a number of factors within each category were examined for their associations with that particular outcome. Table 2, adapted from one presented by Rennie(24, p. 811), indicates the technique of analysis.

Rennie concludes, solely on the basis of inspection of these results, that patients whose age at onset was in the 21-30 decade have a favorable prognosis since this is the modal age category of recoveries from manic-depressive psychoses. It is difficult to follow Rennie in this since we know neither the proportions of 21-30-year-olds in the entire sample, nor the frequency with which this age group is found in any of the other outcome categories. Another study by Rennie(23) of schizophrenic patients followed the same pattern.

Carlson and Rafferty(5) reported a follow-up study of certain flying combat personnel who had incurred anxiety reactions during World War II. A heavy loss of cases makes the authors' statistical analyses hardly necessary since the investigators cannot claim to have a probability sample. This study illustrates a confusion in the criterion of outcome. Instead of the criterion's being concerned with the patient's postmedical-board psychiatric history, it concerns reply or non-reply to the investigator's inquiry. Certain possible predictive factors are examined for their association with this meaningless criterion.

TABLE 2

AGE OF ONSET OF PATIENTS SHOWING LASTING RECOVERY

| Decade of age of onset | 14-20 | 21-30 | 31-40 | 41-50 | 51-60 | 61-70 |
|---|---|---|---|---|---|---|
| Manic depressive psychoses ..... | 13 | 23 | 16 | 13 | 11 | 0 |
| Percentage ..... | 6.2 | 11 | 7.6 | 6.2 | 5.2 | 0 |

Kline and Tenney(17) studied 2,100 consecutive schizophrenic admissions to determine whether a significant correlation existed between prognosis and somatotype. An expert, after examining their photographs, classified the patients into 3 somatotype categories. Because of inability to classify the patient, 28% of the cases were discarded.

Outcome, using the criterion of release without being returned, was determined as of 2 years after the first patient's admission and 6 months after the last admission. Using the chi-square test, significant relationships were found between somatotype and outcome.

The unequal time interval between admission and evaluation of outcome makes the results inconclusive, particularly when we consider the loss of 28% of the cases. The earliest patients had a greater chance of relapsing than the latest patients who were out for only 6 months. It is not unreasonable to assume that there was some degree of mobility in and out of the hospital subsequent to the minimum time of 6 months on trial visit. The variation from patient to patient in the length of time following release could, if taken into account, have been a disturbing factor.

Balinsky(1) attempted to find factors predictive of the posthospital vocational adjustment of schizophrenics. The adjustment of 21 patients was classified as good, fair, or poor, and background data evaluated for their relationship to the criterion. Although predictive value was claimed for certain signs, results based on a handful of cases selected in an unknown manner, and scattered among 3 categories, can not command serious attention.

### STUDIES USING PATIENTS SELECTED FROM AMONG DISCHARGED

Jacob(16) sought to predict, from data which appeared on the commitment form, the readmission or non-readmission of dementia praecox patients furloughed from a state hospital. The writer believes this is an unreliable source of data since it is usually an inadequate document which is completed in haste by people volunteering only the

minimum of information vital to achieve the hospitalization of the patient. Those who feel it necessary to assure the patient's commitment without delay may distort and embellish their report; those who are taking the step with trepidation may give an incomplete or vague report in the hope that they may pave the way for the patient's eventual release. Similar motivations may be present at the time the hospital staff obtains an anamnesis, but the difficulties may possibly be lessened by the interviewer's skill. Jacob did not use the hospital record because of lack of uniformity of data therein. This seems to equate uniformity with reliability. The value of poor data is not enhanced by its being uniformly available, although the collection of such data is facilitated by such uniformity.

Three hundred nineteen dementia praecox patients were selected in some undescribed manner from those who had been furloughed at least 5 years prior to the study. They were subdivided into 4 categories, according to the degree of success following the furlough. The criterion was based simply on status at the time of the study and did not consider the sequential mobility in and out of the hospital in the 5 years subsequent to furlough. For example, a patient out of the hospital for 4 years and 11 months, but hospitalized at the time of the study, was classed as less successful than one who may have been readmitted within 1 month of his furlough, have been hospitalized for 4 years, but was out of the hospital at the time of the study. If success is to be defined in terms of time out of the hospital, a patient's status at the moment of the study may not be used without reference to status changes in the intervening period.

One of the variables studied was condition of the patient on discharge. Consideration of this factor is frequently overlooked. The failure of the statistical manual to operationally define the various conditions makes for wide variation in the application of the definitions, thereby posing special problems for the investigator in prognosis. If one is going to evaluate the effect of certain factors, the condition of the released patients must be controlled in some way.

Jacob's statistical analysis was similar in principle to that of Dunham and Meltzer's. Lack of space does not permit a fuller treatment. It should be noted that Jacob tested his prognostic scale's validity on a new group of cases.

Guttman, *et. al.* (10), followed a series of 188 schizophrenics for 3 years after discharge. A "prognostically favorable" group was selected by excluding patients over 45 years of age and those who had been ill for more than 1 year before admission. The selection was based on the logic that any new method of treatment that could have offered better results than those obtained with the group studied would be definitely justified. This overlooks the possibility that a new treatment might be most effective with either of the kinds of cases excluded from the group studied, or with some particular age or duration of illness category within the total group. The group that is to be used as a control must be more adequately described before its recovery rates can be fruitfully compared with those of any experimental group.

Guttman, *et. al.*, used 5 outcome classes: total recovery, social recovery, social defect, family invalid, hospital invalid. The first 2 categories were grouped, as were the remaining classes and the association of sex and age at onset with these 2 outcome groups was considered. The authors used a chi-square test only in the case of age of onset and found it not to be a significant prognostic indicator. Without testing, it was assumed that sex was associated with the above outcome categories as well as with another dichotomy, namely, relapsed—nonrelapsed patients. Application of the chi-square test to the data of Guttman reveals that sex is not predictive of the outcome categories or relapse and nonrelapse. It is not clear why the chi-square test was used on data whose significance could have been dismissed easily by inspection, but was not applied to data whose significance was in doubt.

Robins(25) attempted to find factors which would make possible, at the time of release, the prediction of the patient's ability to remain out of the hospital for 1 year. Empirical evidence supported the choice of this seemingly arbitrary criterion. It was found

that the number of returns dropped to a mere trickle after 12 months on leave. A success and a failure group, chosen according to a method of stratified random sampling which assured representativeness, at least with respect to age, sex, and diagnosis, was compared for the incidence of certain characteristics. Three groups of variables were investigated. The first group consisted of those associated with the patient's prepsychotic life; the second, those associated with the course of the illness; the third, those associated with the convalescent leave. Previous studies have neglected the third area. The significance of observed differences was statistically tested. Those that gave promise of being significant were retained for multivariate analysis, by means of a statistical technique known as the linear discriminant function. This made it possible to test whether certain patterns of factors were associated with outcome, rather than single factors. There was derived, for each variable, a weight appropriate to the variable's importance in outcome. Although the usual limitations of an *ex post facto* study were present, the research demonstrates careful adherence to scientific principles of investigation. Plans are being made to apply the prediction scale which was developed.

### PROJECTED EXPERIMENTAL DESIGN

The *ex post facto* experimental design operates from the present to the past; the projected design, from the present to the future. Chapin(6, p. 9) describes the latter design as a

> before and after study in which an attempt is made to measure the effects at some future date of the social forces set in motion by some method of social [2] treatment or by a social program, thus following through the flow of events from a present date to some future date.

This might involve an evaluation not only of a particular treatment but also of a set of psychosocial forces to which the patient might have been exposed during his life, *e.g.* marital status, employment, educational achievement. It is possible that factors other than the one whose association with the criterion is being evaluated may be related to outcome. Unless these factors are controlled, the true relationship of the treatment factor may be obscured. The experimental and control groups should be matched on relevant factors, and only the treatment factor or the personal history factors being examined should be varied. Ideally, some method should be used to distribute cases at random into control and experimental groups. This is the best means available to assure that unknown factors which might be related to the criterion are distributed equally in experimental and control groups.

A projected design may make it possible to avoid at least one pitfall of *ex post facto* research, the dependence on old records for obtaining data. Difficulties in setting up a projected experimental design, and the need to wait for results make this type of study rare in prognostic research.

In one such study Low, *et al.*(19), subjected a series of newly admitted patients to a "life situation" test devised by the investigators. The patients were grouped according to their test reactions. Information on outcome which was classified either "in community" or " in hospital" was obtained by questionnaire. A high correlation was claimed between test reaction group and outcome 4 years later. No tests of significance were reported, although application of the chi-square technique by this writer reveals significant differences in outcome among the 5 test reaction groups. The statistical test has meaning only if an unbiased sample can be assumed. Failure of another study by the same authors to corroborate the original results was explained as due to the awareness of the second series of patients that they were being tested.

It seems unfortunate that no advantage was taken in this projected study of the opportunity to obtain information on controlling contingency factors such as differential treatment and posthospital situation. A contribution to the understanding of factors possibly directly associated with outcome was lost.

### MISCELLANEOUS FOLLOW-UP STUDIES

There have been a number of studies that have simply classified the status of patients

[2] Perhaps "psychosocial" should be substituted for "social" in the psychiatric context.

at a particular time after admission, with little or no other information about the characteristics of the patients in each outcome classification. Usually there has been the implicit assumption that the patients studied were a representative sample of all psychotic patients, or of patients with the particular diagnosis studied. Typical have been those done by Bond(4), Bellinger(2), Fuller(9), Osborne(2), Hoffman, *et al.*(13), and Wooton, *et al.*(34).

Hastings, *et al.*(11) ascertained outcome, 5-11 years subsequent to release, of a series of 1,638 admissions. Outcome was classified according to the patient's adjustment in his interpersonal relationships. Diagnosis was the only variable whose association with outcome was measured. The claim made for the uniqueness of the 5 outcome classifications is not supported by the definitions, which are stated in terms of degree of freedom from subsequent hospitalization and symptomatology, not in terms of interpersonal adjustment.

Some studies seem to have been inspired by Kraepelin's contention, cited by Henderson(12), that schizophrenia had a grave prognosis, with only 15% of one subtype making a good recovery. Those who agreed with Kraepelin attempted to reinforce his findings; those who disagreed, to refute them. Many studies failed to define outcome, or did not define diagnosis independently of outcome. "Recovery" may connote one set of behaviors to one investigator and a different set to another. Further, diagnosis of, for example, schizophrenia would be revised if favorable outcome occurred, in which case a high association between schizophrenia and unfavorable outcome would be inevitable.

This is illustrated in a study by Langfeldt (18). He made a differentiation between schizophrenia and schizophreniform psychosis. Reexamination of diagnosis was advised when any case of the latter psychosis showed unfavorable outcome:

> It is true that a number of these [schizophreniform] develop a typical schizophrenia, but a katamnestic revision and careful study of the first symptoms show that also in practically every case the process symptoms referred to could be established. Careful diagnosticians, therefore, should be able to define the most of these cases as typical schizophrenias (18, p. 124).

It would, this writer submits, be difficult for anyone with such a frame of reference to re-evaluate a diagnosis objectively.

Langfeldt attempted to present a standard material for comparison with possible future experimental groups. The sampling method and the failure to describe his population detract from the value of the findings. An attempt was made to follow a series of admissions. When any patient was inaccessible, the next case was chosen until data on 100 cases were obtained. We are not told how many cases were lost in this process, a point which bears heavily on the representativeness of the sample. The cooperation of recalcitrant patients was urged by telling them that "this would enable the clinic to satisfy insurance companies, etc. of the cure effected." Overlooking the ethics of this approach, we might speculate on the validity of responses of patients who knew that research data were to be made available to an insurance company, perhaps a prospective employer.

Israel and Johnson(15), using the technique of statistical reporting they described, are now conducting a massive study of all consecutive first admissions from 1913 to 1950. Outcome is classified by 3 factors: age, sex, and diagnosis. These factors were chosen because they were "objective" and reliably measured. It is further held that there exists a strong organic component in mental disorder to the extent that organic factors provide adequate bases for prediction, overriding other factors. This writer already has expressed his opinion on the importance of evaluating contingency factors. The ultimate test is, of course, the ability of each predictive instrument to decrease the errors of prediction which obtain when only the over-all success or failure rate is used. Crude and refined predictive devices have special uses. If, however, we are searching for a predictive instrument to be applied to an individual case, the more specifically we can place that individual in the prediction matrix, the more confidence we can place in a prediction about his outcome.

Commenting on the value of data about past rates, Wolf(33) points out that "some fail to realize the nature of their assumptions when they treat the chronicle of the

past as an almanac for the future." Revision of the "almanac" is facilitated only when we have studied enough factors to suggest the formulation of further hypotheses concerning the production of a particular effect or outcome.

## Summary and Conclusions

The methodology of a number of prognostic studies has been critically considered, with only incidental reference to the substantive content. The latter has validity only when principles of the scientific method have been applied in deriving and evaluating data. The extensive literature on psychological tests used in prognosis has not been reviewed, since that already has been done by Windle(32).

Most of the research has been found to be so deficient in the use of research procedures as to place in doubt any of the findings. Together with the prognostic statements of the didactic literature, much of the content is of value chiefly because of the conceptual framework and the suggestions they provide of factors whose prognostic relevance should be investigated. These factors have appeared so often in the literature that it seems that sheer force of repetition may have given them an importance not always demonstrated by the specific research reviewed.

Definitive research will be facilitated by a greater degree of interdisciplinary planning. The narrow orientation of particular investigators may have been reflected in the fact that definitions so frequently were not clarified, indicating perhaps the investigator's assumption that no other point of view existed. Little attention, for example, has been paid to contingency factors such as the association of particular furlough situations with outcome. Full coverage of biogenic, psychogenic, and sociogenic factors demands that research psychiatrists join forces with other social scientists, some of whom have been working for years on a related problem, the prediction of recidivism(21).

Research design must be refined. The population must be identified, the sampling process indicated, and the criterion groups defined. All variables under study should be objectively described. Obtaining reliable and valid measures of factors is perhaps a greater problem than the analysis of these measures. Conclusions should be statistically evaluated for significance, and it should be recognized that generalizations apply only to the population sampled. In general, enough information should be reported so that replication would be possible. Prognostic research, which may involve establishing contact with a mobile group of former patients, is expensive. To produce results worth the cost in time and money, strict adherence to scientific procedure is necessary.

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# THE CHANGING CONCEPT OF MAN IN PRESENT-DAY PSYCHIATRY[1]

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We may not overlook the fact that psychiatry was born out of medicine and matured by medicine. By the same token we may not overlook the fact that the psychiatrist, dealing more directly with the inner life of man than the doctor representing any other medical specialty, has always had to draw upon certain special prejudices concerning the human mind, or upon various philosophies prevailing at a given time, in order to create for himself a medicopsychological frame of reference usually not found in autopsy material or purely physiological observations or speculations.

The earliest attempts to form such a frame of reference go back to the thirteenth century and even earlier, when more and more frequent references were being made to human *experience*. These references to human experience came not from the medical men of the time, but from the theologians and philosophers who began to show an increasing interest in human psychology. It is clear therefore that psychology, even empirical psychology, was a product if not an integral part of philosophy. For many centuries it continued to be a fact that if and when a medical man espoused the cause of psychology, he would become a philosopher rather than a psychiatrist. John Locke is a case in point; as late as the seventeenth century psychology was much closer to philosophy than to medicine.

This tradition of thought is rather striking. Thus Pinel, an excellent empirical clinician and practical psychiatrist, himself the son of the rationalist eighteenth century, felt the above-mentioned tradition so deeply that he entitled his *magnum opus* "Nosologie philosophique." The same intellectual tradition remained in force till the end of the nineteenth and the beginning of the present century. William James went through what seemed to be natural evolutionary stages of medicine: physiology, psychology, and philosophy. And Freud, in connection with his gropings for an understanding of the working of the human mind, stated that he did not feel that medicine (*i.e.*, purely biological medicine) interested him very much, that philosophy was a field to which he gravitated more, and he was therefore particularly glad that he developed an interest in psychology which he apparently considered to be intimately connected with philosophy.

In other words, less than 60 years ago the man who was destined to revolutionize our views on human psychology felt that medical psychology was perhaps closer to philosophy than to natural sciences. This, we must note again, came from Freud who did more than anyone else in the history of psychopathology to make psychology a biological discipline, an empirical, scientific system of investigation and understanding of man.

If we cast now a cursory glance at the psychiatrists of the end of the nineteenth century we shall easily notice that people like Meynert, Krafft-Ebing, or Moebius had to draw on the philosophers of their day, had to use them as props as it were to support their theoretical formulations of what they thought man really was. In some respects and in some quarters things are not very different today. Karl Jaspers is an excellent example of how the philosophy of existentialism affects the psychological concept of man—just as Brentano, when Freud was a college student, was an example of how *Gestaltpsychologie* began to show its earliest buds in the circles in which people were turning their attention to psychology.

Freud's was the first psychopathology that seems to have been created independently from a prevailing philosophy. Yet it only seems so. It is true, of course, that despite the apparent similarity of some of Freud's ideas to those of Herbart, Schopenhauer, and Nietzsche, Freud did not read those philosophers till sometime after his basic ideas had been formulated. However, there is incontestable evidence in all the writings of Freud that, while disclaiming any alle-

[1] Read at the 110th annual meeting of The American Psychiatric Association, St. Louis, Mo., May 3-7, 1954.

giance to any established philosophic system, Freud was a Darwinist; he was also deeply under the influence of physics as Helmholz presented it. Therefore, particularly in his earlier writings, Freud seems to show the influence of physicochemical and biophysiological theories which were of his day.

There is nothing unusual about this, of course—except the paradoxical position into which psychoanalysis was put by its own creator: Freud, to whom we owe the knowledge of what Bleuler called depth psychology, this same Freud showed more than a mere tendency to place the understanding of the deepest subjective experiences of the individual into some sort of more or less mechanistic, "objective," disindividualized, scientific Darwinian mold. It is true, Freud never pressed the point so far, and ultimately he seems to have spontaneously liberated himself from the more rigid and more intolerant aspects of what is known as scientism.

To state what I have in mind briefly and without any further elaboration, I would say this: the search for human experience as the source of the understanding of man, a search which started vaguely and obscurely around the eleventh century and was clearly defined by the thirteenth century, reached its peak in the psychoanalytic study of man which I believe is the true successor of the sixteenth-century humanism minus the latter's cultivation of Greco-Roman erudition. This said, the major question or problem which was suggested in the title of this paper may now be approached.

The question is: what concept of man does the psychiatry of today have, if any, and how does the existence of this concept manifest itself in present-day psychiatry?

We need not go into the theological and purely philosophical concepts of man which dominated our thought before the establishment of the so-called scientific age, *i.e.*, the middle of the seventeenth century. Suffice it to say that man has been recognized as a person and a personality since the establishment of the Judeo-Christian tradition. But man as a living value in himself, man as a living unit who in addition to his obligations to God and society has rights which he may assert, man as a functioning unit of mankind—all this was not recognized until the eighteenth century which, through the assimilation of the humanism of the sixteenth century, established the concept of the human personality and man's value as a social particle, as a living individuality, as a moral unit, and as a carrier of life.

Scientific medicine was not much preoccupied with these changing tides of human thought, not because it had no interest in them but because its own tradition sufficed. It was the tradition of saving lives, relieving people from suffering, preserving the normal functioning of the body, preventing illness. All this was sufficient unto medicine, because medicine contained and carried within itself the eternal values of mutual assistance, preservation of life, alleviation of suffering, and rendering man a useful member of the community.

Psychiatry, as is well known, did not fare as well until it liberated itself from many theological preoccupations, until it learned to differentiate soul from psychic apparatus, and to understand that reasoning is not the only weakness of man, that will can be enslaved by a multitude of unconscious drives and emotions. This enlightenment came to us only recently, within our own lifetime. And because it came within our lifetime, it was accompanied by many and hectic battles between the old and the new. The scientific mind was anxious because it saw in depth psychology a possible return to the pre-psychiatric psychiatry of human abstractions. The theologian and the religious moralist were anxious because the new depth psychology seemed to threaten the very foundation of transcendental morality and faith. That a truth, particularly a psychological truth, *is* a truth and therefore cannot threaten but can only support scientific knowledge, that it cannot threaten faith and morality because neither is based on untruth about man—all this seems to have been forgotten during the battle.

The trouble is that a war of ideas, like a war of armies, is really not a contest of reason or reasons. Rather it is, as it probably always shall be, a true war—a combat which is considered really finished only if one of the combatants is destroyed or brought to his knees to ask for mercy. A war of arms cannot end in a draw and bring real peace;

a war of ideas seemingly ending in a draw leaves a sense of ideational vacuum with the passions for mutual destruction temporarily in a faint of fatigue but potentially ready to resume the senseless battle of mutual annihilation any moment an opportunity arises.

We might say that one of the most regrettable human feelings is our inability to keep free from anxiety whenever we are confronted with a new idea, and that the most common defense against such anxiety is hostility, intolerance, incapacity to see the other person's point of view. The anxiety about anything new leads us inevitably to a sort of retreat or regression to older and therefore "safer" habits of thought, and the result is an inevitable loss of energy and creative vision. This I believe is what happened to psychiatry during the last quarter of a century or so. We reopened and reclosed a cycle of mental attitudes which bid fair to affect deeply not only our thinking but our clinical practice.

Some 25 years ago, after Freud had revised his hypothesis as to the origin of anxiety, the human personality seems to have been conceived of (whether a clear-cut formulation of it was made or not) as a complex system of ego defenses against anxiety, and against other noxious psychological agents which are forever interfering with the amorphous peace of the mind. The totality and the indivisibility of the human personality seemed then to have found its psychological, or in the broadest sense of the word its psychobiological, meaning. It did not matter whether one was an "orthodox," "liberal," "eclectic," "free," or "chained" Freudian. Whoever the given clinician was, his was the position that the human personality as a unit and totality was the central point for clinical consideration.

The question, of course, might be raised whether one is justified in ascribing to Freud's influence alone the almost predominant role in the establishment of this twentieth-century humanism in clinical psychiatry. I think we are, for no one more than Freud in the scientific clinical world concentrated our attention on the inner life of man. I have in mind humanism in its traditional historico-psychological sense, not philanthropic humanitarianism—which is a different story and a different department of human endeavor.

It is with the spirit of the new humanism that we entered the last quarter of a century. The optimist had a great deal to be pleased with; the pessimist had to wait a while before he could legitimately raise his voice and point to the rumblings of totalitarian psychology, the mass movements, the almost complete devaluation of human life in the cataclysm of militant fascism and the ensuant world war.

As one reviews the major interests in psychology which developed as a result of these world-shaking events, one cannot help but be impressed with the rather rapid progress of the purely technological approaches to the problem of the functioning of the human personality.

From narcoanalysis (which was not really analysis but rather a form of induced psychological catharsis) to electroshock and cybernetics, the emphasis is laid on the grossly empirical and mechanical and mechanistic. It is really not an exaggeration to say that the process of scientific investigation in clinical psychiatry in the past 25 years has become more and more a process of disindividualization. We have begun again to turn to common denominators among individuals and to inventories and questionnaires. It would be unfair to assume that the above-mentioned procedures are superfluous or without any value in clinical psychopathology; however, it is rather important not to overlook the meaning of the emphasis laid on this or that procedure. In our days we witness the emphasis on inventories, tests, and questionnaires to the detriment if not at the expense of the more direct study of the inner life of the person. And by inner life I do not mean the scientifically undefinable spiritual life of man, but the workings of the psychological apparatus within the living person in the latter's totality.

I am aware that what I am now saying might be considered unjust by some, or plainly untrue by others. For, they might say, is not that which is known as dynamic psychiatry and the almost universal acceptance of the role of the unconscious and of emotions in our psychology—is not this all a true sign of progress? The answer is: it

*is* a sign of progress, but it is not of progress that I speak here; I am speaking of humanism in clinical psychiatry. And it is a recession of our recently acquired humanism that I believe I see. Recession in which sense? Recession in the sense of our overestimation of averages at the expense of the individual; in our tendency to look upon the source of human troubles as lack of adaptation, and at the same time unwittingly but surely and as often as not to confuse adaptation with a sort of psychobiological and sociological conformism. It is a recession of humanism, if under the flag of strict scientific thinking we speak of reality as something which is nothing more than the total perceptual world of man and either overlook psychological reality or confuse it with fantasy, a sort of nonpsychotic delusion.

In the light of these our failings or propensities, we must admit that humanism in its true sense is served poorly or not at all when the individual becomes but a psychobiological adaptive molecule of society, when the individual seems to be looked upon as having been born to serve society, instead of society's having been developed to serve man.

Thus on the purely moral philosophical plane we witness and can recognize the rather sickening phenomenon of the disindividualization of man in favor of his serving the social, or mass machine. No wonder man as a person has been ground up in this machine and has so often come out as so much clay or cement for the use of the master builders of totalitarianism. But what appears to be just as sad, although not so conspicuous, is that on the plane of psychology and psychopathology we are falling victims of the same process of disindividualization, all this to such an extent that even our language has become neologistic and rather mechanistic. We don't live with one another anymore, conveying to one another directly and indirectly our feelings and intuition; we "communicate" instead. We don't talk to one another; we "communicate." We no longer establish relationships to and with people; we "relate" instead—well or badly as the case may be. We don't adjust ourselves to this or that situation; we merely "adjust" well or poorly as the case may be—but intransitively, as it were. We no longer put ourselves in the place of another person, we don't identify ourselves by way of conscious or unconscious psychological processes with this or that person; we omit the word *ourselves* and we plainly "identify"—quite intransitively.

All this, I believe, is the penalty we pay for the mechanistic and organismic point of view which took possession of us under the influence of world wars and global mass movements. Whether this sacrifice of the individual is a temporary one or not remains to be seen, but there is no doubt that the socialization and intellectualization of our attitudes toward man have led to a disindividualized concept of the human personality, even though we might appear in our own eyes more scientific and more accurate if and when we speak in terms of physiology and sociology—as if semantic changes in our vocabulary really enhance our understanding of the meaning and the value of man.

# PREPARATION FOR SPECIALTY CERTIFICATION

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It is a well known fact that most candidates approach the examinations of the American Board of Neurology and Psychiatry with tremendous anxiety. Except for those candidates whose training has given them contact with members of the examining group, examinees have little knowledge of how the examinations will be conducted or exactly how to prepare themselves for the examinations. As a result, much of their five years of preparation is not utilized in the most effective manner. Moreover, much needless tension interferes with their best performance at the examinations.

Having recently experienced this situation, the author is attempting herewith to structure the material of neurology and psychiatry to enable future candidates to improve their study methods and to approach the examinations with greater confidence. There is nothing in this article that is not perfectly obvious and which anyone could not deduce for himself if he were to analyze the situation. However, few of us have the necessary self-discipline to make such analysis before examinations are imminent, which may be too late.

Every examinee has secured a list of questions formerly asked on the examination and has hopefully tried to use them to aid his studies. Such lists are of little value and distract the examinee from more important issues. Examinees do not fail because of the lack of one or two specific answers, provided their general approach to the subject is logical and well-founded. It is fatal to acquire a mass of isolated facts, but to lack the wisdom to integrate them.

The details of the examination procedure and the subject matter included varies somewhat from year to year, and the examinee who prepares himself in detail according to the questions of one year may find himself completely at a loss by the introduction of some new material. Fortunately, by the time the student has reached the postgraduate level he is not studying for details as much as for an awareness of methodology, of progress in research, and of the areas of ignorance in his field.

An outline for preparing for the examinations, then, should not be a list of things to know, but a review of the directions in which to think. They are all self-evident, but none of us stops to map our way clearly and so become lost in a myriad of details.

For this reason the present paper does not attempt to give a listing of the specific questions that may be asked a candidate. It is the author's intent to present the material from a general point of view which will enable the student to integrate his reading and clinical experience into a frame of reference during his 5 or more years of preparation that will provide him with a sufficiently secure foundation that he may approach the day of examination without a major upheaval of the autonomic nervous system.

For Americans an oral type of examination is an unfamiliar experience. The candidate who is nearly ready for boards may waste much time by reading over and over material with which he is familiar. He will discover his weaknesses quickly if he recites his knowledge of a subject out loud to himself or to a colleague. Such rehearsals will give him much confidence in meeting his examiners.

## NEUROANATOMY

Candidates should review neuroanatomy from the following viewpoint.

1. Dissect a brain or study a large papier-mâché model, spending some time every day until "sudden insight" fixes the relationships indelibly. Learn the blood supply and the relation of the cranial nerves to the major masses.

2. Trace incoming impulses to their destination. In the cord at first try to learn only the major ascending (posterior column, lateral, and ventral spinothalamic, dorsal and ventral spinocerebellar) and the major descending groups (direct and indirect pyramidal).

3. Trace outgoing impulses.

4. Eventually learn the principal central interconnections, adding details only later.

5. Review basic neuroanatomy each time a clinical test is performed until it is "fixed."

Neuroanatomy is a subject that every student finds difficult. The dissection of the brain in medical school is done at a time when the student has so little background for understanding the details that he retains relatively little. We have all had the experience of reviewing neuroanatomy and forgetting it within a short time. Admittedly there is no substitute for the urgency of the examinations to motivate the student to memorize the essential details.

It is possible, however, to learn neuroanatomy in such a manner that it "sticks." The first important thing is to get some key 3-dimensional relationships fixed in the mind so that the major masses and pathways are clearly understood. The only way in which to do this securely is to dissect one, or preferably several, brains. One is amazed to discover, after a few hours of studying an actual brain with the aid of diagrams and a papier-mâché model, that what had hitherto seemed an insoluble puzzle suddenly becomes an organized whole.

When the student arrives at the point when he can go to the autopsy room and quickly identify the cerebral vessels, the surface markings, the major masses and the cranial nerves, he need no longer fear neuroanatomy. Until he has gained this proficiency it is foolish to waste time studying the details and trying to memorize the labels on the cross-section of the mesencephalon.

The earlier in his career the specialist fixes forever in his mind the fundamentals of neuroanatomy, the easier will be his study of neuropathology, clinical neurology, psychosurgery, and neurophysiology.

In learning the cross-section of the spinal cord the student should restrict himself to a few of the most important tracts clinically, namely the pyramidal tracts, the spinothalamic, the dorsal and ventral spino-cerebellar and the posterior columns. If he learns to spot these on any section of the cord, later other details will fall into place without effort. Similarly in the brain stem, it is useless to try to learn all the details of a cross-section. One must think in terms of origin and termination of the fibers and try to locate nuclear masses in terms of some already learned, external landmark.

The ultimate purpose of learning by rote the basic neuroanatomical relationships is to gain facility in tracing a response to a stimulation from its source to its central termination, and the associations of the central nuclei through which appropriate responses are made. *The student who tries to avoid the initial tedium of learning a few fundamental systems will waste hours of effort.*

The number of paths by which stimuli reach the cortex is very limited; the student should write them down on a piece of paper and trace them on a diagram, on a model, on an actual brain dissection, and finally on his own mental image. Then he should concern himself with the interconnections between the major areas of the brain. This is more complex and he should attempt at first to learn only the major interconnections, following which he will have an adequate basis for understanding the more obscure connections. Psychosurgery can be conveniently reviewed at this time.

The student is wasting his own time who does not review to himself the mediating nervous pathways of every clinical test he performs. He should choose a text illustrated in such a manner to give him clear visual images which he can later recall. He will find it very helpful to use diagrams in which all parts of one system are delineated in the same color from cord to cortex.

Many problems in neuroanatomy will be solved for the student who spends a few hours reviewing neuroembryology. All the illogical complexities of the brain have a developmental logic; once this is understood the hopeless task of rote memory is replaced by comprehension.

The problems of neuroradiology can be resolved if the student studies neuroanatomy with a skull at hand. One may learn all the details of the soft tissue and remain ignorant of their relations to the bony parts. The student thinks he knows this from medical school days, but the likelihood is that he does not realize the immensity of his own ignorance. At the present requirements the candidate who does not have contact with a radiologist can teach himself the fundamentals of neuroradiology from a good text.

## Clinical Neurology

The following is a suggested guide for review of clinical neurology:

1. Take every opportunity to study neurological cases from the point of view not only of diagnosis but of the systems involved.
2. Each time neurological examinations are done, review to self the sensory and motor pathways involved.
3. The psychiatrist need not be infallible in his neurological diagnosis but he must understand the logic behind his examinations.
4. Memorize one or two key symptoms and pathological features of each entity around which details can later be organized.
5. Answer the question, "What else could cause this?"
6. Gather all the raw signs and symptoms before in terms of diagnosis.
7. Think in terms of the laboratory pathology: electroencephalogram, histopathology, gross pathology, the x-ray, blood chemistry, etc.

The average student finds the subject of clinical neurology a large one to digest. He will save himself much time by making a few tedious efforts to put the subject into a logical pattern in his own mind. After he has thoroughly grounded himself in neuroanatomy he is in a position to think of neurological disease in terms of the structures involved. This simple order will solve many of his problems since knowledge of the involved structures will often give him the cue to the basic symptoms.

The number of structures that can be involved in neurological disorders is finite and the student who knows neuroanatomy knows them. The number of types of disturbance that can occur to these structures is also finite and the student should make a list of them. He will then realize that this is no mystical realm of medicine, but a collection of facts which can be reduced to an outline.

Reading of clinical neurology should always be done with the mind open to the memory page on which is listed the anatomical areas and the types of pathological processes. Obviously, in this study clinical neurology and neuropathology are inseparable. One thoughtful inspection of a specimen of brain showing lesions of the globus pallidus resulting from carbon monoxide poisoning will teach more anatomy, function, pathology, and symptomatology than pages of text.

There is much overlapping in the symptomatology of neurological disorders, and it is illogical to attempt to learn a complete listing for each diagnostic category. It is best to learn the distinctive symptom for each disease and to later fill in details. Moreover, certain symptoms which are secondary to involvement of some major system should be learned as "pyramidal tract involvement" or "symptoms of increased intracranial pressure" rather than as an unorganized group of details.

For the psychiatrist, a correct diagnosis of his neurological patient at the time of examination is less important than his method of eliciting the facts and interpreting their significance. To this end the psychiatric resident should perfect a technique of neurological examination, knowing the reason for each test and the significance of all the possible pathological deviations. If he can do this, and can elicit a meaningful history, he has little to fear.

## Neuropathology

For the study of neuropathology the following outline for review is suggested:

1. Make a list of the basic types of pathological reaction in the central nervous system.
2. Review each case that you study clinically with the basic physical pathology in mind.
3. Take every opportunity to make detailed study of patients coming to autopsy.
4. Learn the statistically most common conditions thoroughly.
5. In micropathology learn the type of detail revealed by the major stains.

Neuropathology is a push-over to the student who has made a logical approach to the subject. Again, he can make no progress until he has thoroughly acquainted himself with functional neuroanatomy. He must first familiarize himself with the types of pathology that can occur in the nervous system. These again are finite, can be written on a piece of paper, and committed to memory. When the student has achieved this first logical approach to the subject his worries are nearly over.

He should avail himself of every oppor-

tunity to observe pathological specimens, always considering first the general point of view of whether the lesion is degenerative, inflammatory, neoplastic, etc. Once he is firmly founded in these general principles, the details of microscopic appearance will gradually assume logic.

In viewing specimens of gross pathology the student should train himself to observe the details in some convenient logical sequence so that when confronted with an examination "unknown" he will have a systematic approach by which to evaluate the specimen. This will gain him more credit than a correctly guessed diagnosis which he can not substantiate on closer questioning. If the individual is thoroughly familiar with normal gross anatomy and can rapidly check the blood vessels, the meninges, the white, the gray, the gyri and sulci, the ventricles, the symmetry, etc., he will have little difficulty learning to recognize the major pathological conditions. To the candidate who has no system, a specimen that is perfectly normal except for the congenital absence of some structure will prove baffling. If the candidate can describe in technical vocabulary what he sees he will usually have little difficulty.

Microneuropathology may prove difficult, especially for the candidate who has received no formal training. His first approach must be to learn the basic staining methods and to know which structures will be delineated by the stains. He must know that a H & E or a Nissl stain will show him cell structure and not myelin sheaths. He must learn to think that if he is given a cross section of cord in H & E stain he should look at the cells of the central gray or of the meninges. A quick look at the slide grossly will tell him whether there is infiltration of the meninges; if there is he will think first of tuberculosis and look for giant cells, etc. If there is not he will think first of polio and check the anterior horn cells, etc.

If the candidate is given a myelin sheath stain, he knows at once that most neurological conditions are eliminated from his consideration and he concentrates on those in which myelin is affected. Incidentally, he should not be surprised to have his knowledge of neuroanatomy tested at this point when the examiner asks what functional disturbance he would expect from the lesion shown him!

## Basic Psychiatry

For the study of basic psychiatry, the following points will be extremely helpful:

1. There is no substitute for a biographical familiarity with historical figures.
2. As a student progresses he should become familiar with various theoretical formulations.
3. Think of differential diagnoses in clear-cut, well-defined categories.
4. Remember this is a changing field which tomorrow's research may alter.
5. Always think in terms of what the current research problems are and the significant recent findings.
6. Read as though you were a teacher and make up your own questions.
7. Spend plenty of time alone without books in quiet review of the knowledge you already possess.
8. Reduce the dynamic patterns in the various neuroses to a basic formula and later add details.
9. Make lists of important terms and test your ability to discuss them.

The examination in basic psychiatry crucially evaluates the candidate's mental maturity. One may have read profusely without acquiring a useful working knowledge of his material. Unless he utilizes systematic approach to his reading, the student will waste hours and may never achieve mastery of his subject. Two things are necessary: one is some tentative theoretical frame of reference to which the ideas of others can be compared, and second, a thorough knowledge of the basic psychiatric literature against which later innovations can be compared.

The student who tries to plunge into a study of comparative schools of psychiatry and psychology today without being grounded in Kraepelin, Bleuler, and Freud will be in a position as hopeless as that of an anatomist who knew nothing of embryology. For example, the work of Sullivan loses half its significance to the student who does not understand Sullivan as an evolutionary sequence.

The student will save himself time in the

long run by reading the original papers of universally agreed significance. For one thing he will be asked if he has read some of them at the examination. More importantly, if he reads the case of Little Hans, or Bleuler's monograph, these papers will become a more integrated part of his thinking than a dozen summaries of such papers. Having read the basic material, subsequent reading goes much faster because the student has more facts by which to make associations.

Early in his review the student should read a survey book such as Clara Thompson's *Psychoanalysis, Evolution and Development* or Mullahy's *Oedipus: Myth and Complex*. The student should think in terms of the one or two singular contributions of the various great names. For example, in studying comparative psychoanalytic theory, he should not become lost in a mass of detail, but at first attempt to recall only the major issues over which dissenters disagreed with the orthodox school. Once these are learned, details will accrete without effort.

Conclude each session of reading with a verbal summary of the material. This sharpens comprehension and improves verbal facility. It is helpful to put a large sheet of paper on the wall on which to write notes, list of names belonging to a certain "school," chronological sequences, etc. So diagrammatized, the material assumes a tangibility that facilitates memory.

The candidate must be able to associate names with ideas, to identify the authors of important terms, and to associate names with the important current researches.

Naturally, the student must be familiar with the neurophysiology of the autonomic nervous system, thalamus, and hypothalamus.

The candidate must ground himself in the fundamental principles of the somatic therapies, the theories of their mode of action, the history of their development, their indications, contraindications, and limitations.

The candidate should be familiar with various aspects of psychoanalytic and psychotherapeutic treatment. He should know the important modifications in the various techniques, the reasons for them, and the objections to them. Most important he should have a clear, consistent conception of his own treatment methods and goals.

## Clinical Psychiatry

For successful study of clinical psychiatry, remember the following rules:

1. In addition to making a diagnosis by positive signs and symptoms, also think in terms of the features that make other diagnoses impossible.
2. Think in terms of your theoretical formulation of the etiological factors.
3. Think in terms of the symbolic significance of features of the symptomatology.
4. Think in terms of treatment.
5. Think in terms of the possibility of an organic lesion that could cause the symptomatology.

Obviously there can be no separation between basic and clinical psychiatry and this section refers merely to the formalities of the examination.

The apprentice psychiatrist learns too quickly certain basic criteria by which to attach a label to a patient. If he spends his apprenticeship by satisfying himself with making a diagnosis on the basis of one or two clinical findings he will learn very little.

On the other hand, if the apprentice makes a detailed differential diagnosis with every case listing the features that favor his final selection and the features which by their presence or absence exclude other considerations he need not fear the examination in clinical psychiatry, provided, of course, he shows the requisite courtesy and maturity in interviewing his patient that any candidate would expect of himself. The examinee is being tested on his method of handling whatever situation confronts him and should not become anxious if he fails to elicit a complete history in the 20 minutes available.

The candidate may expect to be questioned on material included in the section on basic psychiatry during a large part of his clinical hour.

# CEREBRAL ATROPHY IN PSYCHIATRIC PATIENTS[1]

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In clinical psychiatry the problem frequently arises of whether a patient is suffering from a "functional" neurosis or psychosis, or from such a syndrome symptomatic of a progressive cerebral atrophy. Whereas the disorientation, memory loss, incontinence, and other late signs of the "organic" mental syndrome are easily recognized as a rule; in the early stages the diagnosis is difficult. Yet knowledge of the presence of cerebral atrophy is often important in evaluating the patient's prognosis and his probable response to therapy.

Aside from examination of brain tissue, which is usually not feasible in the living patient, the best diagnostic test for establishing the diagnosis of cerebral atrophy is the pneumoencephalogram, which shows increased air in the subarachnoid spaces, and dilated ventricles. (The pneumoencephalogram, of course, will not differentiate the more specific cerebral atrophy syndromes, *e.g.*, Pick's disease, Alzheimer's disease, arteriosclerotic and senile brain disease, one from another.) There is considerable evidence, however, to suggest that these syndromes form something of a continuum and are difficult to separate clearly, not alone by radiological, but also by clinical(1, 2) or pathological(3, 4) means. Most important to the psychiatrist is the presence or absence of a progressive brain disease.

The present study was designed to add information concerning clinical phenomena that are more or less positively correlated with finding of cerebral atrophy in the pneumoencephalogram. The clinical data on a series of cases in which the pneumoencephalogram did reveal atrophy were compared with those in which the air injection, done because of suspicion of cerebral atrophy, revealed normal findings. It is hoped that this comparison will further clarify the clinical characteristics associated with cerebral atrophy and aid in the judgment as to which patients may be expected to show a pathological pneumoencephalogram.

[1] From the Boston Psychopathic Hospital and Department of Psychiatry, Harvard Medical School.

## MATERIAL AND METHODS

Two groups of cases were used: (1) All those in the period 1943–1953, hospitalized at the Boston Psychopathic Hospital, in which the pneumoencephalogram was satisfactory, and was read as "moderate to marked cerebral atrophy" on the basis of increased subarachnoid air and ventricular dilatation. The amount of air used varied from 70 cc. to 170 cc. There were 40 such cases, 23 female, 17 male. (2) All those cases from the same period, in which the diagnosis of cerebral atrophy was suspected, but in which the pneumoencephalogram, showing good filling of ventricular and subarachnoid spaces, was considered as normal. (Cases were not used in which the pneumoencephalogram was done as a routine procedure, *e.g.*, as part of the study prior to prefrontal lobotomy.) There were 29 cases used in this group, 15 male, 14 female, which will be referred to as "control" cases.

For both groups the following data were taken from the charts; age, clinical history including initial mental symptoms noted, mental status on admission, neurological findings and results of cerebrospinal fluid, electroencephalographic and psychometric examinations when available. Also in the absence of the characteristic "organic mental syndrome" the indication for pneumoencephalography was recorded.

## RESULTS

*Initial psychiatric symptomatology.*—The first prominent mental symptoms noted in 15 (37.5%) of the patients with subsequently proven atrophy was memory loss of some degree. In the remaining 25 (62.5%) the history began in a manner less characteristic of brain disease. The kinds of early mental symptoms noted in this latter group may be broken down as follows:

In 6 patients (15%) paranoid delusions,

with or without hallucinations, appeared early in the illness. The following is a characteristic case summary.

A.K.—49-year-old woman, 5 years prior to admission, while pregnant, refused to admit her pregnancy, began accusing neighbors of being German spies. She developed a bad temper, became combative, and during the subsequent years gradually became more confused, developed aphasia and urinary incontinence.

In 6 other patients (15%), depression, frequently with suicidal tendencies, was the initial psychiatric picture, as in the following case:

M.H.—36-year-old woman, following her mother's death, became progressively more depressed and agitated, stayed in bed and refused food. Six months later she developed urinary incontinence, became disoriented and was hospitalized.

Seven patients (17.5%) began with marked personality changes—irresponsibility, neglect of family, repeated vagrancy or drunkenness offenses. The remaining 6 patients (15%) showed early behavior which would be classified as neurotic—anxiety attacks, obsessiveness, mild depressions, and irritability. One such case summary follows.

H.S.—55-year-old man, was always considered "odd." Five years prior to admission he was noted to be excessively irritable at small frustrations, and to be abnormally jealous of his wife. Within the next few years he developed somnolence, forgetfulness, and speech disturbances.

These findings are summarized in Table 1.

*Total incidence of memory loss in patients with cerebral atrophy.*—In 21 of the 40 patients, there was on admission a definite history elicited of memory loss developing at some time prior to the patient's hospitalization; of the control group, in 3 there was such a history.

The mental status of 20 of the 40 patients on admission revealed some degree of memory loss. It was found in 7 of 29 (24%) of the controls.

Taken together, memory loss, as part of the history or as found on clinical examination, occurred in 28 of the 40 patients with atrophy (70%) and in 8 of 29 controls (27.5%).

*Age.*—The number of patients of different age groups, with cerebral atrophy, compared with those of the control group, is seen in Table 2.

It will be seen that 10 (25%) of the patients with atrophy were under 40, and only 3 were under 30. Where the pneumoencephalogram was negative, however, 21 patients (72%) were under 40, and 12 (41.5%) were under 30. This would seem to indicate that cerebral atrophy as seen in a mental hospital population, although suspected fairly frequently in the younger age group, is uncommon below the age of 40.

Eight of the 10 cases who did show atrophy, and were under 40, presented gross disorientation or memory loss; 5 had a history of previous serious head injury, and 5 had a history of excessive alcoholism, whereas of the control group of 21 cases under 40, 4 were found to have memory loss or disorientation, and one patient had a history of head injury.

*Hypertension and Head Injury.*—Blood pressures in excess of 150 systolic and 100 diastolic were found at some time during hospitalization in 5 of the patients with cerebral atrophy and in 5 controls.

A definite history of head injury was obtained in 5 patients with cerebral atrophy, only one of whom, however, had mental symptoms immediately following the injury. History of head injury was obtained in 2 controls.

*Neurological Signs.*—Thirty-five of the 40 patients (87.5%) as compared with 6 of 29 controls (20.5%) either had convulsive seizures or showed some abnormality on neurological examination. The kind and in-

TABLE 1

INITIAL MENTAL ABNORMALITIES NOTED IN PATIENTS WITH SUBSEQUENTLY PROVEN CEREBRAL ATROPHY

| | |
|---|---|
| Memory loss | 15 |
| Paranoid ideas | 6 |
| Depression | 6 |
| Conduct disorders (psychopathy) | 7 |
| "Neurotic" disturbances | 6 |

TABLE 2

| Age | 10-20 | 21-30 | 31-40 | 41-50 | 51-60 | 60 plus* |
|---|---|---|---|---|---|---|
| Patients with cerebral atrophy | 1 | 2 | 7 | 9 | 19 | 2 |
| Controls | 5 | 7 | 9 | 4 | 4 | |

* The hospital did not routinely admit patients over 60; this fact explains paucity of patients in this age group.

cidence of neurological signs is indicated in Table 3.

Gait disturbances, the most common neurological symptom, were usually those of ataxia, staggering and wide-based gait, but in a few cases there was a *marche à petit pas,* and in one a spastic gait. Tremors were usually rhythmical, fine and bilateral, occasionally choreiform. Reflex abnormalities refer only to asymmetry of deep tendon reflexes, or the presence of a Babinski sign. Speech disturbances, other than aphasia, were usually dysarthria, occasionally stuttering or abnormal movements of the muscles associated with speech.

No neurological abnormalities were recorded on examination in 8 patients with cerebral atrophy (20%) and in 19 controls (65%). A fine tremor of outstretched hands was the only neurological sign in 4 patients with atrophy and in 2 controls: if this is not considered pathological, the neurological examination was negative in 12 patients (30%) and in 21 controls (72%).

*Cerebrospinal Fluid.*—Total protein was elevated over 45 mgm% in 11 patients found to have cerebral atrophy and in 10 controls.

*Findings in patients lacking the "organic" mental syndrome.*—Particular interest attaches to those patients with cerebral atrophy in whose clinical history memory loss was not brought out, and in whom it was not found on examination. There were 11 such patients (27.5%). In 5 the clinical symptomatology was chiefly paranoid in type, with delusions and hallucinations; all 5 had prominent neurological abnormalities. In 5 other patients the psychiatric syndrome was depression—in some agitated, in some retarded. In contrast with the preceding group, in these 5 the neurological examination was normal. Pneumoencephalogram was done in 3 of these cases because the pattern of results on psychometric examination was that of organic brain disease, in one because of a grossly abnormal EEG, and in the last because the patient responded to electric shock treatment by becoming wildly disoriented. The case summary of one of these depressed patients follows:

M.K.—52-year-old woman, 2 years prior to admission began to be depressed and inactive after the death of her husband and the discovery that her son had multiple sclerosis. Prior to this time she had been a dependable, easy-going and hard-working person. On admission she was noted to be negativistic, agitated, and appeared to be deeply depressed. Neurological and cerebrospinal fluid examinations were normal. She was given electric shock treatment, developed urinary incontinence, and became completely disoriented. At this time the pneumoencephalogram was done.

*Reasons for suspicion of cerebral atrophy in the control cases.*—Sixteen of the group with normal pneumoencephalograms (55%) gave no history or evidence of memory loss, nor showed any abnormalities on neurological examination (as compared with 12.5% of the group showing cerebral atrophy). The findings of these control patients leading to the air study were: elevated cerebrospinal fluid protein (7 cases), performance on psychometric test suggestive of brain damage (3 cases), grossly abnormal EEG (4 cases), and combination of the latter 2 factors (2 cases).

*Comment.*—Data used in this study have 2 major limitations which are acknowledged: (1) The diagnosis of cerebral atrophy by radiological means is admittedly inaccurate, and mistakes are frequently made both in false positive and false negative readings. Error may well have occurred in the "control" group (calling pneumoencephalograms normal when subarachnoid spaces were not filled sufficiently to demonstrate atrophy). It is felt, however, that by using only cases diagnosed by the radiologist as "moderate to advanced" atrophy, error on the other side (calling a normal pneumoencephalogram pathological) was minimized. (2) The clinical histories obtained of these patients

TABLE 3

| | Patients | % | Controls | % |
|---|---|---|---|---|
| Gait disturbances | 13 | 32.5 | 2 | 6.9 |
| Aphasia | 12 | 30 | 1 | 3.4 |
| Abnormal spontaneous movements (tremors, etc.) | 9 | 22.5 | 5 | 17 |
| Seizures | 9 | 22.5 | 2 | 6.9 |
| Reflex abnormalities | 9 | 22.5 | 1 | 3.4 |
| Speech disturbances (other than aphasia) | 7 | 17.5 | 1 | 3.4 |
| Incoordination and intention tremor | 6 | 15 | 1 | 3.4 |
| Pupillary abnormalities | 5 | 12.5 | 0 | |
| Paresis of arm or leg | 4 | 10 | 0 | |
| Facial asymmetry | 3 | 7.5 | 0 | |
| Unilateral sensory disturbances. | 2 | 5 | 0 | |
| Retinopathy | 2 | 5 | 0 | |

varied, of course, in reliability and completeness. There is no way to avoid the source of inaccuracy, but it does not affect the comparison of data from histories of patients with pathological with those with normal pneumoencephalograms.

That all kinds of brain disease may early manifest themselves by a variety of psychiatric phenomena has of course long been known, and has been extensively documented in the cases of general paresis(5). This study merely reemphasizes the difficulty in the early diagnosis of cerebral atrophy by pointing out that, in many cases, paranoia, depression, or neurotic personality disturbances overshadowed or preceded the onset of dementia. The relationship of these early symptoms to the brain tissue loss is of course a difficult and subtle question.

It is recognized that the cases collected here under the diagnosis of "cerebral atrophy" have various etiologies. Several are probably cases of Alzheimer's disease and Pick's disease; some probably have cerebrovascular disease, a few may have some relation to previous head trauma, and there may be a few cases of multiple sclerosis(6), Huntington's chorea (7), and rare syndromes such as olivopontocerebellar atrophy (8). There is, however, little reason to think that these different pathological processes responsible for cerebral atrophy give different psychiatric syndromes, and this psychiatric similarity of the end-product of cerebral atrophy is the justification for dealing with the cases as a uniform group.

The clinical characteristics of those patients subsequently shown to have cerebral atrophy by x-ray are in somewhat sharp contrast to those with normal pneumoencephalograms with respect to age, mental status, and neurological findings. The presence of cerebral atrophy is rendered less probable by the existence of any one of 3 factors: (1) youth—particularly below 40; (2) absence of history of demonstrable memory loss on examination; (3) normal neurological examination. With the presence of a combination of any 2 of these factors in one case, cerebral atrophy is rendered more improbable, and the necessity of performing a pneumoencephalogram, an uncomfortable procedure with some risk, should be carefully considered in such cases.

Suspicion of other brain disease than cerebral atrophy, e.g., space-occupying lesions, may of course warrant pneumoencephalography, and it should be commented in passing that the differential diagnosis of other organic diseases of the nervous system is not here dealt with. There are some patients with brain tumors, for example, who present a gradually developing behavioral disturbance which is a problem to diagnose as "organic" brain disease. Their number is small; the predictive criteria as to air study outlined here generally apply to this group also and, in addition, there are usually some signs or symptoms of increased intracranial pressure.

At times, however, the presence in a young person of an atypical unclassifiable psychosis, combined perhaps with EEG abnormality and some defect in performance on psychometric testing, leads to a pneumoencephalogram. As has been seen, the odds against the demonstration of cerebral atrophy in such a case are very great. The following is an illustrative case.

K.H.—35-year-old man, had always been irritable, solitary, and unscrupulous. Within the past few months he had felt depressed, had had insomnia, and had shown marked instability in work history. On examination he was alert, oriented, self-derisive, erratic in speech. Psychometric examination revealed discrepancies in performance like that seen in patients with brain damage. Electroencephalogram showed some diffuse abnormalities. Neurological examination was negative, as was the pneumoencephalogram.

In the older age group, however, there were 5 cases with no marked memory impairment and no marked neurological abnormalities, who showed cerebral atrophy on the pneumoencephalogram. All these individuals were clinically depressed. In the depressions of older age groups, then, the foregoing predictive criteria as to the results of the air study are less valuable.

## Summary and Conclusions

1. The early clinical symptoms of 40 patients with cerebral atrophy subsequently demonstrated by pneumoencephalogram are reviewed. In 25 cases the earliest abnor-

malities noted were not memory loss or impairment, but paranoid ideas, depression, neurosis, or psychopathy.

2. Clinical data of the patients with cerebral atrophy were compared with those of a control group who were suspected of having cerebral atrophy, but had normal pneumoencephalograms. The diagnosis of cerebral atrophy is found to be positively correlated with 3 factors: (a) presence of memory impairment by history or examination, (b) presence of abnormal findings on neurological examination, and (c) age over 40.

With the absence of any 1 of these factors the probability of demonstrating cerebral atrophy radiologically is considerably diminished and with the absence of any 2 the probability becomes extremely small.

3. Cerebral atrophy was found in 5 patients with depression, of the 40-60 age group, without evidence of memory impairment or abnormalities in the neurological examination.

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# PSYCHIATRIC PATIENTS LOOK AT OLD AGE: LEVEL OF ADJUSTMENT AND ATTITUDES TOWARD AGING[1, 2]

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The rapidly growing proportion of middle-aged and elderly persons in our population has focused attention on the problems of aging and the ideas held by various groups about old age. Review of the literature, Morgan (4), Landis (3), Tuckman and Lorge (5), on attitudes about aging emphasizes the generally negative outlook that most people have toward the later years of life. All 3 investigations dealt with people who showed no mental disturbances.

The present study has as its focus: (1) to determine the general attitudes of the mentally sick toward aging; (2) to examine the relationship between level of adjustment and attitudes toward aging; and (3) to compare these with the reported findings on normal subjects.

## METHOD

### ADMINISTRATION

To facilitate comparison with normal subjects, a questionnaire, identical to the one used by Tuckman and Lorge with graduate students, was administered to 2 groups of patients at the Winter V. A. Hospital. The questionnaire touched on 21 aspects of living, *e.g.*, "freedom from worry," "living a full life," "authority and prestige," etc. The patients were asked to rank these aspects (*e.g.*, when they thought people were most free from worry, had least authority and prestige, etc.) among 8 age periods ranging from childhood through old age. The life span was divided as follows: childhood (up to 12 years); adolescence (from 13 to 19 years); the age span from 20 on was divided into decades, the last category being 70 years and over.

As an example of the directions and method of administration used in the questionnaire, the aspect of "authority and prestige" was presented as follows:

AUTHORITY AND PRESTIGE

*Directions:* People have more authority and prestige at certain times in their lives than at others. After that period in life when you think people have the most authority and prestige, write the number 1; after that period which takes second place in this respect, write the number 2; until finally you write the number 8 after that period when you think people have least authority and prestige.

| | | | |
|---|---|---|---|
| Up to 12 years | —— | 40 to 49 years | —— |
| 13 to 19 years | —— | 50 to 59 years | —— |
| 20 to 29 years | —— | 60 to 69 years | —— |
| 30 to 39 years | —— | 70 years and over | —— |

The full list of aspects of living to be ranked by the patients was as follows:

| | |
|---|---|
| Happiness | Salary |
| Freedom from Worry | Activity in Family Affairs |
| Financial Security | Taking Part in Active Sports |
| In the Swing of Things | Friends |
| Freedom | Clubs and Organizations |
| Health | Hobbies |
| Interest in Religion | Ambition |
| Living a Full Life | Ability to Learn |
| Authority and Prestige | Interest in Politics |
| Needed by Others | |
| Meaningfulness of Life | |
| Job Satisfaction | |

Patients were instructed that the rankings were to be accomplished by having "people in general" in mind rather than themselves necessarily. The average time required to complete the questionnaire was about 30 to 40 minutes. In addition, the patients were asked to answer the question "What does old age mean to you?" with a paragraph or two.

### SUBJECTS

The subjects comprised 2 groups of patients. One consisted of 38 acutely disturbed closed ward patients in partial remission and the other of 47 open ward patients diagnosed as psychoneurotic and character and behavior disorders. Tables 1, 2, and 3 give the relevant information concerning the backgrounds of both groups. Table 1 indicates that pa-

[1] A portion of this paper was read at the 1953 meeting of the American Psychological Association, Cleveland, Ohio.

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TABLE 1

DISTRIBUTION OF DIAGNOSTIC CATEGORIES IN CLOSED WARD AND OPEN WARD PATIENTS

| Category | Proportion of closed ward patients % | Proportion of open ward patients % |
|---|---|---|
| Schizophrenic reaction, paranoid... | 79 | |
| Schizophrenic reaction, unclassified | 14 | |
| Involutional psychosis ..... | 5 | |
| Schizophrenic reaction, catatonic.. | 2 | |
| Anxiety reaction ..... | — | 20 |
| Somatization reaction ..... | — | 15 |
| Depressive reaction ..... | — | 15 |
| Asthenic reaction ..... | — | 9 |
| Inadequate personality ..... | — | 10 |
| Passive-aggressive personality .... | — | 9 |
| Antisocial personality ..... | — | 9 |
| Schizoid personality ..... | — | 9 |
| Emotional instability reaction..... | — | 4 |
| | 100 | 100 |
| Number of cases..... | 38 | 47 |

TABLE 2

MEANS, STANDARD DEVIATIONS, AND RANGES FOR INTELLIGENCE, AGE, AND EDUCATION OF CLOSED WARD AND OPEN WARD PATIENTS

| Measure | Closed ward patients (N = 38) | Open ward patients (N = 47) |
|---|---|---|
| IQ | | |
| Mean ..... | 108.2 | 103.6 |
| SD ..... | 14.2 | 20.9 |
| Range ..... | 70–131 | 62–133 |
| Age | | |
| Mean ..... | 34.5 | 37.5 |
| SD ..... | 9.7 | 11.4 |
| Range ..... | 20–61 | 26–61 |
| Education | | |
| Mean ..... | 11.1 | 10.7 |
| SD ..... | 2.6 | 4.2 |
| Range ..... | 5–16 | 3–20 |

TABLE 3

OCCUPATIONAL BACKGROUND OF CLOSED WARD AND OPEN WARD PATIENTS

| Occupation | Proportion of closed ward patients % | Proportion of open ward patients % |
|---|---|---|
| Professional ..... | 11 | 12 |
| Clerical ..... | 26 | 26 |
| Skilled ..... | 29 | 28 |
| Unskilled ..... | 16 | 17 |
| Farmer ..... | 18 | 17 |
| | 100 | 100 |
| Number of cases..... | 38 | 47 |

tients with schizophrenic reactions, paranoid type, dominate the closed ward population, and that the anxiety reaction category followed by the somatization and depressive reactions make up the major groups in the open ward. Table 2 shows that both groups are well matched on the variables of intelligence, age, and education. They are slightly above average in intelligence, have mean ages in the 30's, and have completed close to 3 years of high school. Table 3 shows that skilled and clerical occupational backgrounds principally characterize both groups of patients.

## RESULTS

At each age period, mean ranks were determined for both groups for each of the 21 listed aspects of living. Separate analyses were carried out for those under and over 35 years in both groups, but in neither did reliable differences appear between the ranks assigned to the various age periods. This finding agrees with that of Tuckman and Lorge who also found no significant differences between the mean ranks assigned the various age periods by graduate students (mean age, 31.2 years) under and over 30 years of age. As a consequence, only total group determinations are reported in the present study.

Tables 4 and 5, respectively, show the average ranks assigned by the closed ward and open ward patients to the 8 age periods for the 21 aspects of life adjustment. A coefficient of concordance, W(1), which provides an estimate of the degree of agreement between subjects on each aspect is also included. W can vary from 0 to 1; the larger the W, the higher the degree of agreement.[4] In both groups, the coefficients of concordance for most aspects differ substantially from zero. However, there is a wide range of concordance—from .01 to .86 in the closed ward group and from .02 to .86 in the open ward group. In both groups there was greatest agreement in ranking the aspects of "taking part in active sports," "health," and "ability to learn,"; least agreement for the

[4] $W = \frac{12S}{m^2(n^3-n)}$ where S = sum of squares of deviations from mean $\frac{m(n+1)}{2}$; m = number of judges; and n = number of judgments.

## TABLE 4

MEAN RANKS OF AGE PERIODS FOR 21 ASPECTS OF LIFE ADJUSTMENT BY 38 CLOSED WARD PATIENTS, TOGETHER WITH CONCORDANCE MEASURE IN THESE RANKINGS

| | Mean rank for age periods | | | | | | | | Concordance in rank |
|---|---|---|---|---|---|---|---|---|---|
| Aspect of life adjustment | Up to 12 | 13-19 | 20-29 | 30-39 | 40-49 | 50-59 | 60-69 | 70 and over | W |
| Happiness | 2.7 | 3.2 | 4.0 | 3.9 | 4.5 | 5.5 | 5.9 | 6.3 | .28 |
| Freedom from worry | 1.5 | 2.9 | 4.4 | 5.0 | 5.2 | 5.6 | 5.6 | 5.7 | .30 |
| Financial security | 4.6 | 4.7 | 4.2 | 3.6 | 3.6 | 4.5 | 5.0 | 5.9 | .10 |
| In the swing of things | 6.2 | 4.3 | 2.6 | 2.2 | 3.3 | 4.7 | 5.7 | 7.1 | .50 |
| Freedom | 3.1 | 2.8 | 3.3 | 4.7 | 5.2 | 5.6 | 5.7 | 5.5 | .25 |
| Health | 2.9 | 2.2 | 2.3 | 3.6 | 4.8 | 5.8 | 6.7 | 7.7 | .72 |
| Interest in religion | 4.4 | 4.7 | 4.6 | 4.5 | 4.9 | 4.5 | 4.3 | 4.2 | .01 |
| Living a full life | 6.1 | 4.8 | 3.3 | 2.6 | 3.2 | 4.1 | 5.2 | 6.7 | .36 |
| Authority and prestige | 6.9 | 5.9 | 4.5 | 3.3 | 3.0 | 2.8 | 3.7 | 5.7 | .39 |
| Needed by others | 3.7 | 4.2 | 4.3 | 4.3 | 4.5 | 4.7 | 5.1 | 5.2 | .04 |
| Meaningfulness of life | 7.1 | 5.6 | 3.6 | 2.6 | 3.2 | 3.9 | 4.7 | 5.3 | .36 |
| Job satisfaction | 7.2 | 5.2 | 3.4 | 2.4 | 3.0 | 3.7 | 4.9 | 6.3 | .47 |
| Salary | 7.5 | 6.1 | 3.9 | 2.5 | 2.2 | 2.7 | 4.6 | 6.7 | .70 |
| Activity in family affairs | 6.1 | 5.3 | 3.0 | 2.7 | 3.1 | 4.1 | 5.1 | 6.6 | .37 |
| Taking part in active sports | 3.6 | 1.3 | 2.1 | 3.6 | 4.8 | 5.8 | 6.8 | 7.9 | .86 |
| Friends | 4.9 | 3.4 | 3.6 | 4.0 | 4.2 | 4.6 | 5.3 | 5.9 | .13 |
| Clubs and organizations | 6.5 | 4.4 | 3.3 | 2.8 | 3.2 | 4.1 | 5.1 | 6.6 | .35 |
| Hobbies | 5.8 | 3.9 | 3.7 | 3.5 | 4.1 | 4.3 | 5.0 | 5.8 | .13 |
| Ambition | 5.3 | 3.2 | 1.7 | 2.6 | 4.1 | 5.3 | 6.5 | 7.5 | .66 |
| Ability to learn | 2.7 | 2.3 | 2.5 | 3.5 | 4.6 | 5.6 | 6.8 | 7.9 | .75 |
| Interest in politics | 7.4 | 6.1 | 3.6 | 2.2 | 2.6 | 3.5 | 4.6 | 6.1 | .59 |

## TABLE 5

MEAN RANKS OF AGE PERIODS FOR 21 ASPECTS OF LIFE ADJUSTMENT BY 47 OPEN WARD PATIENTS, TOGETHER WITH CONCORDANCE MEASURE IN THESE RANKINGS

| | Mean rank for age periods | | | | | | | | Concordance in rank |
|---|---|---|---|---|---|---|---|---|---|
| Aspect of life adjustment | Up to 12 | 13-19 | 20-29 | 30-39 | 40-49 | 50-59 | 60-69 | 70 and over | W |
| Happiness | 3.0 | 2.6 | 3.5 | 3.5 | 4.3 | 5.3 | 6.5 | 7.3 | .49 |
| Freedom from worry | 1.2 | 2.6 | 4.3 | 4.8 | 5.0 | 5.5 | 6.1 | 6.5 | .54 |
| Financial security | 5.2 | 5.5 | 5.1 | 3.8 | 3.1 | 3.8 | 4.4 | 5.1 | .12 |
| In the swing of things | 5.4 | 3.9 | 2.4 | 2.6 | 3.4 | 4.7 | 6.2 | 7.2 | .48 |
| Freedom | 4.2 | 3.7 | 3.2 | 3.9 | 4.6 | 5.0 | 5.3 | 6.1 | .15 |
| Health | 2.6 | 1.9 | 2.5 | 3.7 | 4.8 | 5.9 | 6.8 | 7.9 | .82 |
| Interest in religion | 5.9 | 5.2 | 5.6 | 4.4 | 4.0 | 3.9 | 3.7 | 3.5 | .14 |
| Living a full life | 5.1 | 4.0 | 3.1 | 2.8 | 3.6 | 4.7 | 5.8 | 7.0 | .34 |
| Authority and prestige | 7.5 | 6.2 | 4.2 | 2.9 | 2.4 | 2.8 | 4.3 | 5.8 | .56 |
| Needed by others | 3.8 | 3.7 | 4.4 | 4.4 | 4.6 | 4.7 | 5.1 | 5.3 | .05 |
| Meaningfulness of life | 7.0 | 5.4 | 4.3 | 3.1 | 3.3 | 3.5 | 4.3 | 5.1 | .28 |
| Job satisfaction | 7.1 | 5.4 | 3.6 | 2.3 | 2.6 | 3.4 | 4.9 | 6.5 | .53 |
| Salary | 7.3 | 5.7 | 3.8 | 2.4 | 2.1 | 3.1 | 4.8 | 6.6 | .63 |
| Activity in family affairs | 6.1 | 5.0 | 3.1 | 2.2 | 2.9 | 4.3 | 5.6 | 7.0 | .47 |
| Taking part in active sports | 3.8 | 1.4 | 2.1 | 3.4 | 4.6 | 5.8 | 6.9 | 7.9 | .86 |
| Friends | 4.8 | 3.4 | 3.3 | 3.4 | 4.0 | 4.9 | 5.6 | 6.5 | .22 |
| Clubs and organizations | 6.9 | 4.8 | 3.4 | 2.3 | 2.9 | 4.1 | 5.3 | 6.3 | .32 |
| Hobbies | 6.4 | 4.5 | 3.9 | 3.5 | 3.1 | 3.5 | 4.8 | 6.2 | .02 |
| Ambitions | 5.0 | 2.7 | 2.0 | 2.8 | 4.3 | 5.2 | 6.4 | 7.7 | .65 |
| Ability to learn | 3.7 | 1.8 | 2.3 | 3.3 | 4.6 | 5.6 | 6.8 | 7.8 | .74 |
| Interest in politics | 7.8 | 6.6 | 4.6 | 2.5 | 2.1 | 2.8 | 4.2 | 5.7 | .70 |

aspects of "needed by others" and "financial security." In addition, the closed ward patients showed high agreement in ranking the aspect of "salary" and little agreement in ranking "interest in religion"; the open ward patients, high agreement in ranking "interest in politics" and little agreement in ranking "hobbies."

Inspection of Tables 4 and 5 discloses that the age periods chosen as most and least favorable vary for the different aspects. The closed ward patients consider the 30's as generally most favorable, followed by adolescence, and then childhood. The open ward patients regard adolescence, the 20's, 30's, and 40's as almost all equally favorable. It is interesting to note that after the 40's no age period is considered as most favorable for any aspect except the 70's and over for "interest in religion," and the 50's for the aspect of "authority and prestige" by the closed ward patients. In the main, both groups rank the young adult years, adolescence, and childhood more favorably than any of the later year periods. These findings are also in substantial accord with the results of Tuckman and Lorge.

The closed ward patients seem to think of childhood as a time when people are happy and unworried, and needed by other persons; adolescence is *par excellence* the time of life for taking part in sports, for health, learning, being free, and having friends; the 20's, the period when ambition is at its peak; the 30's, when people are really in the swing of things, living the full life, active in family affairs, and most interested in politics; the 40's, when people make the most money; the 50's, when they have the greatest prestige and authority; the 70's and over, when interest in religion becomes paramount. The picture is much the same for the open ward patients, but there are differences. The open ward patients consider adolescence rather than childhood as the happiest time of life and one when they are most needed by others; the 20's, when people are really free and in the swing of things; the 40's, when they are most interested in politics, have greatest authority and prestige, are financially secure, and go in most for hobbies. It is conjectured that many of the patients interpreted the aspect of "needed by others" to mean "a need for others." On the whole, the possible effects of lack of reality contact and bizarre interpretations were surprisingly minimal. It should be recognized that the questionnaire approach used in this study most likely taps "public attitudes," conscious and relatively superficial reactions reflecting social conformity, more than it does the "deep layers" of the personality.

To facilitate further comparison with the student population of Tuckman and Lorge, the 21 aspects were categorized as follows: physical and psychological functioning—"health," "taking part in active sports," and "ability to learn"; employment—"salary," "job satisfaction," and "ambition"; status and social participation—"activity in family affairs," "friends," "needed by others," and "authority and prestige"; activities and interests—"interest in politics," "clubs and organizations," "interest in religion," and "hobbies"; general values—"happiness," "freedom," "living a full life," "financial security," "freedom from worry," "meaningfulness of life," and "in the swing of things."

All 3 groups show similar patterns for those aspects involving physical and psychological functioning, values, and status and social participation. They place the median peak age period for physical and psychological functioning in adolescence; for values, in the 20's; and for status and social participation, approximately in the 30's.

With regard to employment, both groups of patients place the median peak in the 30's. The students, by contrast, put it in the 40's.

The largest disparity in median peak ranking is with respect to those aspects involving activities and interests, except for "interest in religion" which all groups place in the 70's and over period. The closed ward patients place the median peak period in the 30's; the open ward patients, in the 40's; the students, in the 50's. Although the closed ward patients rank the median peak age period for activities and interests earlier than do the open ward patients, both groups of patients place it earlier than do the students.

Following the example of Tuckman and Lorge, the data were additionally analyzed by considering the age period given rank 1 (most favorable) or rank 8 (least favorable). The findings substantiate those already indicated. The data reveal that both closed ward and open ward patients regard the early and

intermediate years of life more favorably than the later ones. This is also the finding for the students.

Fifty-eight to 87% of the closed ward patients consider childhood and adolescence as the most favorable periods for aspects like "freedom from worry," "taking part in active sports," "ability to learn," "happiness," and "freedom." Fifty-six to 93% of the open ward patients see these age periods as most favorable for "freedom from worry," "taking part in active sports," "health," "ability to learn," "happiness," and "needed by others."

Sixty-six to 84% of the closed ward patients and 56% to 88% of the open ward patients rank the 20's, 30's and 40's as the most desirable periods for a majority of the aspects—"in the swing of things," "living a full life," "meaningfulness of life," "job satisfaction," "salary," "activity in family affairs," "clubs and organizations," "ambition," and "interest in politics."

Only a small percentage in either group consider the period from 60 and on as favorable for any aspect except "interest in religion." Fifty percent or more of the closed ward patients regard the age period of 70 and over as least favorable for the aspects of "happiness," "health," "living a full life," "taking part in active sports," "ambition," and "ability to learn." In addition to these 6 aspects, over 50% of the open ward patients also consider this period as least favorable for "freedom from worry," "in the swing of things," and "friends." Actually, for some aspects, fewer than 5% consider any age period beyond the 20's and 30's as most favorable. This is true for the closed ward patients with respect to "taking part in active sports" after the 20's, and for "ambition" after the 30's; for the open ward patients, for "taking part in active sports" after the 20's and for "health" and "freedom from worry" after the 30's.

Nevertheless, some aspects reveal an opposite trend. For example, fewer than 20% of the closed ward patients consider the 70's and over as least favorable for "interest in religion" and "interest in politics." In the open ward group, fewer than 20% see this period as least favorable for "financial security," "meaningfulness of life," "authority and prestige," "interest in religion," and "interest in politics."

On the choice of childhood as the most happy period of life, a comparison of our findings with those mentioned in the literature indicates that the obtained figure of 47% for the closed ward patients is higher than those reported. Morgan and Landis (2) found that 14.5% and 11.1%, respectively, of their subjects (65 to 98 years of age) chose childhood (from 5 to 15 years) as the most happy period. Tuckman and Lorge found that 35% of their students ranked childhood as the most happy age. Part of the difference between the figure obtained for the closed ward patients and those reported by Morgan and Landis may be due to the age differences in the populations involved and possibly divergent conceptions of the meaning of happiness. With regard to the Tuckman and Lorge data, their figure of 35% is close to the 32% obtained for the open ward patients. The stress placed by the closed ward patients on childhood as the most happy period may express the keenly felt discrepancy between their present status and a time when life seemed less complex and offered more hope. Although the relationship is certainly not striking nor necessary, one might speculate about the possible relevance of this finding to concepts like fixation and regression.[5]

The emphasis by the patients on the early years of life as most happy can be more clearly seen when the periods of childhood and adolescence are combined. Sixty percent of the closed ward and 62% of the open ward patients view these years as the most happy. For these same periods, Tuckman and Lorge report 47%; Morgan and Landis, 33.4% and 30.4%, respectively, even though the latter 2 investigators considered the period of adolescence to extend up to 25 years of age.

With reference to the early years of adulthood (24-45 years) in this connection, Landis and Morgan reported that 51.4% and 49.1% of their people, respectively, ranked this period as the most happy one. When the data were prorated, results for this similar period were approximately 32% for the

[5] Dr. Robert R. Holt brought this interesting possibility to my attention.

Tuckman and Lorge population, 28% for the open ward, and 20% for the closed ward patients. The data imply that, at the time of ranking, a majority in all groups considered the most happy years of life as already having passed. It is probably true that these rankings reflect status at the time of responding—half of the older people were receiving old age assistance, the students were faced with, as yet, unaccomplished goals, and the rest were patients in a hospital. Nevertheless, despite these varying conditions, the directional trend in regarding the younger years of life as more happy than the later ones is the same for all the groups.

With regard to employment, it is intriguing to note that many patients do not necessarily equate job satisfaction with salary. Rather, they seem to believe that when maximum salary is achieved, job satisfaction begins to decline. For example, the largest group of open ward patients, 38%, chose the 30's as the most favorable period for "job satisfaction"; only 17% considered this same period as most favorable for "salary." On the other hand, the largest group of open ward patients, 43%, chose the 40's as the most favorable period for "salary"; but only 21% considered this same period as most favorable for "job satisfaction."

Table 6 summarizes the various response categories obtained from both patient groups in answer to the question "What does old age mean to you?" Percentages for each category and the reliability of the differences between them are indicated. The reliability of the response categories was determined by having an independent scorer also categorize the answers. The agreement was 91% indicating that classification of the responses was carried out with a high degree of consistency.

TABLE 6

SIGNIFICANCE OF THE DIFFERENCES BETWEEN RESPONSE CATEGORY PERCENTAGES OF CLOSED WARD AND OPEN WARD PATIENTS TO THE QUESTION "WHAT DOES OLD AGE MEAN TO YOU?"

| Category | Proportion of closed ward patients % | Proportion of open ward patients % | Significance of differences between closed and open ward patients |
|---|---|---|---|
| Inactivity | 26 | 4 | .01 |
| Have to depend on others | 16 | 17 | N.S.* |
| Restful period | 3 | 20 | .01 |
| Life has little meaning | — | 4 | NS.. |
| Wisdom and maturity | 11 | 6 | N.S. |
| Don't know—haven't reached that stage yet | 13 | 6 | N.S. |
| Death approaching; the end | 26 | 20 | N.S. |
| Physiological decline | 5 | 23 | .01 |
| | 100 | 100 | |
| Number of cases | 38 | 47 | |

* No significant difference at the .05 level of probability.

Table 6 also reveals that the closed ward patients view old age mainly as a period of inactivity when one is no longer able to accomplish much (26%), becoming dependent on others (16%), and heralding the approach of death (26%). In similar vein, the open ward patients consider the later years of life essentially as a period of physiological decline (23%), when you have to depend on others (17%), and foreshadowing the end of life (20%). Nevertheless, a definite minority in both groups of patients regard the later years in a more positive light. Eleven percent of the closed ward and 6% of the open ward patients think of old age as a period when one can wisely bring to bear the accumulated experiences of a lifetime. Also, 20% of the open ward patients and 3% of the closed ward patients see old age as a time when life becomes restful and easy because one can reap the rewards of his life's work. It is noteworthy, that 13% of the closed ward patients and 6% of the open ward patients blocked on or avoided directly answering the question. To some extent, this probably mirrors the difficulties that many patients have in dealing with future events symbolically.

With respect to significant differences between the patient groups, at the 1% level, the closed ward patients significantly more often view old age as a period of inactivity, and the open ward patients significantly more often regard it as a time of physiological decline, and, also, as a restful period. Seemingly, even though many of the open ward patients stress the physiological decline which occurs in old age, they do not feel that this necessarily leads to inactivity; or, it may be, that implied in the concept of physiological decline is the idea of inactivity thus making the disparity between the groups on this point much less than appears. In like manner, it is possible that one explanation for

the discrepancy between the groups concerning old age as a restful period may be that implicit in the closed ward patient rankings of inactivity is the idea of taking it easy and resting in the later years of life.

## Summary and Conclusions

1. It is quite obvious, from the favorable positions accorded childhood, adolescence, and the early years of maturity as contrasted with the negative perspective held toward the later years of life, that both groups of patients generally view old age with a gloomy eye.

2. The degree of mental disturbance in the patients has little seeming effect on their over-all saturnine attitude toward old age.

3. In both the closed ward and open ward patients, there appears to be little association between the age of the patients and their outlook on aging. This agrees with the reported findings in the literature for "normal" groups. It is notable that both groups of patients, with mean ages in the 30's, regard this age period to be most favorable for the aspects of "living a full life," "meaningfulness of life," "job satisfaction," "activity in family affairs," and "clubs and organizations"; also, that many of them felt that for those in this age period the most favorable years for aspects like "happiness," "freedom from worry," "health," "ability to learn," "friends," and "ambition" have already passed by.

4. The negative orientation of the patients to the older years of life is also consonant with the attitudes reported in the literature for various "normal" segments of the population. The conspicuous feature of this finding is that so little difference can be attributed to mental illness *per se*, of either moderate or severe degree. It seems that attitudes toward old age, at least on the level tapped by the questionnaire method used in this study, are heavily determined by a widespread social attitude or ideology, rather than by idiosyncratic experiences.

It is often said that our culture highly values youth and sees little to admire or look forward to in old age. The findings of this study give substance to these impressionistic statements by sociologists. They emphasize the extraordinary degree of uniformity in attitudes toward aging which is maintained, even in the most deviant members of the community, as far as their mental health is concerned.

5. There should be little doubt concerning the need for an educational program to train people for adjustment to old age. It could have the broad aim of anticipating and preventing the anxieties and maladjustments attendant on growing old. We possess mounting evidence that age changes are not uniform and that distinct individual differences exist; not only are there well-established inter-individual variations, but intra-individual ones as well. In addition, data increasingly suggest that the manner in which the culture treats and reacts to aging, as well as the person's own concept of what it means to grow old, may be responsible for more psychological difficulties than the aging process *per se*. An honest reevaluation of our thinking concerning old age is called for.

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# INSULIN COMA THERAPY

## A Study of Results in an Army Hospital [1]

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One of the most difficult problems in modern psychiatry is the evaluation of the results of treatment, whether psychotherapeutic or somatic. The author had charge of the insulin coma ward of the neuropsychiatric service at a large army hospital for almost a year during 1953 and 1954. The following is a study of the results of this method of treatment since October 1951, when it was begun here, to the present time (June 1954).

It is well known that the problems involved in scientific evaluation of this form of therapy are great indeed. In a recent critique on insulin treatment in schizophrenia, David(1) states the following:

> Any attempt to evaluate the considerable psychiatric literature dealing with IST (Insulin Shock Treatment) in schizophrenia is complicated by a seeming lack of research sophistication, as evidenced by such problems as:
>
> (1) Unstandardized criteria for the initial selection of patients for IST. Frequently, acute and chronic cases are mixed into the same group, thereby contaminating the findings.
>
> (2) Unstandarized criteria for the assessment of changes in clinical status and the general neglect of reporting concomitant physiological observations make it difficult to evaluate studies by different investigators.
>
> (3) Failure to use properly matched control groups to gauge the importance of factors other than IST. Not infrequently summaries are too gross to permit adequate comparisons.
>
> (4) Reporting conclusions either without stating the . . . time elapsed between completion of treatment and assessment of clinical status or without awaiting results from an adequate follow-up study.

We feel, parenthetically, that this treatment is correctly termed insulin coma therapy, not insulin shock therapy, common usage to the contrary notwithstanding. It is not "shock" in the sense that an electrical "shock," or convulsion is produced, nor is clinical medical shock induced, in the sense of decreased circulating blood volume, hypotension, etc. The goal of each treatment is a deep coma, with no "shock" in any sense desirable.

To return to David's critical remarks on insulin research: we shall indicate how we have attempted to approximate ideal conditions for the evaluation of our series:

We found that 89 patients had had a full course of insulin coma therapy since October 1951. Many more had begun treatment and had been dropped for various reasons. A "full course" meant 30 or more comas (not treatments) in 68 cases, and between 20 to 30 comas in 21 cases. Patients with fewer than 20 comas were not considered to have had a full course.

We first matched our insulin-treated group with a control group by diagnosis only, not considering chronicity or severity of illness. The control groups were selected entirely at random from among patients contemporary in the hospital with the insulin group, with no consideration of any other forms of therapy, except that the controls did not get insulin.

The diagnosis were as follows: In the insulin coma group—89 patients (100%): schizophrenia, paranoid, 54 patients (61%); schizophrenia, catatonic, 15 patients (17%); schizophrenia, latent, 3 patients (3%); schizophrenia, simple, 2 patients (2%); schizophrenia, mixed, 12 patients (14%); other, 3 patients (3%).

In the control group—89 patients (100%): schizophrenia, paranoid, 55 patients (62%); schizophrenia, catatonic, 16 patients (18%); schizophrenia, latent, 3 patients (3%), schizophrenia, simple, 2 patients (2%); schizophrenia, mixed, 12 patients (14%); other, 3 patients (3%).

The sex, color, and age matchings of the 2 groups were as follows:

*Sex:* All patients in both groups were males.

*Color:* In the insulin coma group (89 patients), 60 (67%) were white; 15 (17%), negro; 14 (16%), unknown. In the control group (89 patients), 60 (67%), white; 16 (18%) negro; 3 (3%) other; 10 (11%), unknown.

[1] From Madigan Army Hospital, Tacoma, Wash.

*Age:* Insulin coma group (range: 19-45): age 19, 4 patients (4%); age 20-29, 69 patients (78%); age 30-39, 8 patients (9%); age 40-45, 2 patients (2%); unknown 6 patients (7%). Control group (range: 17-45): age 17-19, 4 patients (4%); age 20-29, 70 patients (79%); age 30-39, 11 patients (12%); age 40-45, 3 patients (3%); unknown, 1 patient (1%).

The chronicity of illness in both series was as follows: insulin coma group: acute, 37 patients (42%); chronic, 48 patients (54%); unknown, 4 patients (4%). Control group: acute, 35 patients (39%); chronic, 53 patients (60%); unknown, 1 patient (1%).

The matching of our insulin patients with controls, then, so far as diagnosis, sex, color, age, and chronicity of illness are concerned is as close as could possibly be done. Only in the severity of illness is there a significant difference in the 2 groups: a larger number of insulin-treated patients were rated severely ill as compared with the controls.

*Severity of Illness.*—Insulin coma group: severe, 57 patients (64%); moderate, 25 patients (28%); mild, 2 patients (2%); unknown, 5 patients (6%).

Control group: severe, 45 patients (51%); moderate, 36 patients (40%); mild, 7 patients (8%); unknown, 1 patient (1%).

*Electroshock treatment.*—It is interesting to note that even in the matter of whether the patient had had a course of electric shock therapy the 2 groups matched well. (Most of the insulin patients who had EST had it before insulin was instituted.) These figures are as follows: Insulin coma group EST, 50 patients (56%); no EST, 33 patients (37%); unknown, 6 patients (7%).

Control group: EST, 51 patients (57%); no EST, 36 patients (40%); unknown, 2 patients (2%).

## RESULTS

*Criteria for the assessment of change.*—Our criteria in both groups are clinical criteria only. The clinical record of each patient was studied for a statement of his mental status at the time of final disposition. In addition, the actual practical disposition was noted—whether he returned to full military duty, or, at the other extreme, whether he was transferred to a Veterans Administration Hospital. Five categories of change were then established:

0. Unimproved, and transferred to a Veterans Administration Hospital.

1. Slightly improved and transferred to a Veterans Administration Hospital.

2. Slightly improved and discharged to own custody or to family.

3. Improved, much improved, or in complete remission and discharged to own custody.

4. Improved, much improved, or in complete remission and returned to military duty.

In our evaluation of clinical change the date of the evaluation, *i.e.*, the time of disposition, varied from one to 6 months after the termination of insulin treatment in the insulin groups, the average time being 2 to 3 months. Tentative efforts were made to assess results in both insulin and control groups after an adequate follow-up study, but in the military setting it was soon found that it was impossible to trace our patients very far after they left the hospital. To find our patients a year, for example, after they left the hospital, would require extended efforts by many social agencies all over the country, and, in any case, we would probably lose such a large number of patients as to make the efforts not worth while. This study involves short-term results, then, but includes *every* member of the series, where all members have been evaluated in a standard manner by a fairly constant psychiatric staff. Results of treatment are shown in Tables 1 and 2.

A study of the results of treatment, by diagnosis, shows that about twice as many patients diagnosed catatonic improved with-

TABLE 1

RESULTS OF TREATMENT

| Category of improvement | Insulin group | | Control group | |
|---|---|---|---|---|
| | No. of patients | Percentage of improvement | No. of patients | Percentage of improvement |
| 0 | 19 | 21 | 15 | 17 |
| 1 | 12 | 14 | 12 | 14 |
| 2 | 18 | 20 | 18 | 20 |
| 3 | 31 | 35 | 35 | 39 |
| 4 | 7 | 8 | 9 | 10 |
| ? | 2 | 2 | 0 | 0 |
| Total | 89 | 100 | 89 | 100 |

TABLE 2

RESULTS BY DIAGNOSIS, WITH NUMBERS OF PATIENTS CHARTED

| Diagnosis | Total | | Category of improvement | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | 0 | | 1 | | 2 | | 3 | | 4 | | ? | |
| | I | C | I | C | I | C | I | C | I | C | I | C | I | C |
| Schizophrenia, paranoid | 54 | 55 | 13 | 12 | 6 | 7 | 11 | 11 | 19 | 20 | 5 | 5 | 0 | 0 |
| Schizophrenia, catatonic | 15 | 16 | 3 | 0 | 5 | 2 | 1 | 2 | 6 | 12 | 0 | 0 | 0 | 0 |
| Schizophrenia, latent | 3 | 3 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 1 | 1 | 1 | 0 | 0 |
| Schizophrenia, simple | 2 | 2 | 0 | 0 | 0 | 0 | 1 | 2 | 1 | 0 | 0 | 0 | 0 | 0 |
| Schizophrenia, mixed | 12 | 10 | 3 | 3 | 1 | 1 | 4 | 3 | 3 | 1 | 1 | 2 | 0 | 0 |
| Unknown | 3 | 3 | 0 | 0 | 0 | 1 | 0 | 0 | 1 | 1 | 0 | 1 | 2 | 0 |
| Total | 89 | 89 | 19 | 15 | 12 | 12 | 18 | 18 | 31 | 35 | 7 | 9 | 2 | 0 |

I = Insulin Coma Patients.
C = Control Patients.

out insulin coma therapy, compared with the same diagnostic group who got insulin coma therapy. There were no significant differences of any other kind in the results, broken down diagnostically.

There were, similarly, no significant differences between the 2 groups considered as a whole. If anything, the insulin-treated group showed a somewhat larger percentage of no improvement whatever, 21% as compared with 17% for the controls. The control group showed slightly better results in categories 3 and 4.

*Average length of Hospitalization by Category of Improvement.*—Insulin coma group (7.1 months average, for total group.) —Category 0, 8½ months average; 1, 8½ months average; 2, 6⅓ months average; 3, 7 months average; 4, 6½ months average. Control group (4.2 months average, for total group.)—Category 0, 5½ months average; 1, 4⅓ months average; 2, 3⅓ months average; 3, 4⅔ months average; 4, 3 months average.

One of David's criticisms of insulin research was the frequent mixing of acute and chronic cases, contaminating the findings. Table 3 gives a detailed analysis charting categories of improvement against (1) treatment with EST (2) chronicity (3) severity of illness for both insulin and control groups.

These findings tend to show: (1) EST alone, appears to be as effective, if not more effective than insulin alone or in combination with insulin. (2) Improvement in the acutely ill group occurs with the same rate for both insulin and control groups. (3) Similarly, among chronically ill patients there was no significant difference in the rate of improvement between the 2 groups. (4) Significantly more seriously ill patients are tried on insulin. The percentage of improvement for them, however, is the same as in the control group.

We feel we have complied with proper insulin research techniques, as outlined by David.

In table 3 we have charted results of acute and chronic cases separately.

TABLE 3

RESULTS OF TREATMENT FOR ACUTE AND CHRONIC CASES

| | | Total group | | | | 0 and 1 | | | | 2 | | | | 3 and 4 | | | | ? | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | I | | C | | I | | C | | I | | C | | I | | C | | I | | C | |
| | | No. of patients | | No. of patients | | No. of patients | | No. of patients | | No. of patients | | Percent of total | | Percent of total | | Percent of total | | Percent of total | | Percent of total | |
| EST | Total | 89 | 100 | 89 | 100 | 31 | 35 | 27 | 31 | 18 | 20 | 18 | 20 | 38 | 43 | 44 | 49 | 2 | 2 | 0 | 0 |
| | Yes | 50 | 56 | 51 | 57 | 27 | 54 | 19 | 37 | 4 | 8 | 5 | 10 | 19 | 38 | 27 | 53 | 0 | 0 | 0 | 0 |
| | No | 33 | 37 | 36 | 40 | 2 | 6 | 8 | 22 | 14 | 42 | 13 | 36 | 17 | 52 | 15 | 42 | 0 | 0 | 0 | 0 |
| | ? | 6 | 7 | 2 | 2 | 2 | 34 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 34 | 2 | 100 | 2 | 34 | 0 | 0 |
| Chronicity of Illness | Acute | 37 | 42 | 35 | 39 | 4 | 10 | 5 | 14 | 4 | 11 | 3 | 9 | 29 | 79 | 27 | 77 | 0 | 0 | 0 | 0 |
| | Chronic | 48 | 54 | 53 | 59 | 26 | 54 | 22 | 41 | 14 | 29 | 15 | 28 | 8 | 17 | 16 | 31 | 0 | 0 | 0 | 0 |
| | ? | 4 | 4 | 1 | 2 | 1 | 25 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 25 | 1 | 25 | 2 | 50 | 0 | 0 |
| Severity of illness | Severe | 57 | 64 | 45 | 50 | 30 | 52 | 21 | 47 | 7 | 12 | 4 | 9 | 20 | 36 | 20 | 44 | 0 | 0 | 0 | 0 |
| | Moderate | 25 | 28 | 36 | 40 | 0 | 0 | 6 | 16 | 11 | 44 | 12 | 33 | 14 | 56 | 18 | 50 | 0 | 0 | 0 | 0 |
| | Mild | 2 | 2 | 7 | 8 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 30 | 2 | 100 | 5 | 70 | 0 | 0 | 0 | 0 |
| | ? | 5 | 6 | 1 | 2 | 1 | 100 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 40 | 1 | 20 | 2 | 40 | 0 | 0 |

I = Insulin Coma Patients.
C = Control Patients.

1 Our criteria for clinical change were largely practical: did the patient return to duty, or did his hospitalization continue elsewhere? Also, the standardized statements of condition at time of disposition universally used in psychiatric hospitals were accepted, although each clinical record in both insulin and control groups was carefully studied for corroboration or negation.

2 Our groups were extremely closely matched with regard to diagnosis, sex, age, color, and chronicity of illness. Insulin treated cases tended to be more severely ill than the controls.

3 The lapse of time between termination of treatment and evaluation was the maximum possible in our setting. Either we had an evaluation of results at this time or no evaluation at all.

## Summary and Conclusions

Insulin coma therapy has been used at this hospital since October 1951. Up to June, 1954, 89 patients had received a full course of therapy. A very closely matched group of control patients was obtained, with following conclusions:

1. The insulin coma group showed essentially the same number of improved patients as the control group. Figures for the control group were actually slightly better.

2. The results in the group diagnosed catatonic schizophrenia were much better for the control group than for the insulin treated group. There was no difference in the other diagnostic categories.

3. Where EST was used it was at least as effective as insulin coma therapy.

4. Insulin does not increase the rate of improvement in patients diagnosed either acutely or chronically ill.

5. Although more severely ill patients are given insulin coma therapy, their improvement rate is the same as the control group.

It is our conclusion that the insulin coma therapy has been of little value in itself in the improvement of patients who have had it. We would venture to state that the results we have obtained, which only equal the results of a very similar group not getting insulin, were due to somatic and psychotherapeutic efforts simulataneous with the insulin coma therapy. Psychiatric hospitalization averaged 3 months longer per patient in the insulin group. Our conclusions do not warrant this excess period in the hospital.

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# HISTORICAL NOTES

## EARLIEST USE OF THE TERM DEMENTIA PRÆCOX

Dr. B. A. Morel, physician-in-chief to the Asylum for the Insane, Saint-Yon (Seine-Inférieure) in his *Traité des Maladies Mentales* (1860), pp. 565-6, has the following passage:

One of my unhappy memories is of an example of morbid heredity in a family with the members of which I had been brought up in my youth. An unfortunate father consulted me one day about the mental condition of his son, aged 13 or 14. This lad, having previously shown only the most loving attitude toward his father, had suddenly manifested violent hatred of him.

The child had a well-formed head and his intellectual faculties were notably superior to those of his school fellows. What struck me at once was his underdevelopment physically. This was the subject of his first complaints although it could hardly itself account for the peculiar symptoms he presented. He felt humiliated by being the smallest boy in his class, although he was always first in his studies, and that without effort and with little actual work. He seemed to grasp his lessons by intuition and he retained them easily in memory.

Gradually he lost all sprightliness, became gloomy and taciturn and inclined to seek seclusion. At first the suspicion of self-abuse was entertained but it proved unfounded. The melancholy state deepened and the boy's hatred for his father reached the point where he thought of killing him. His mother was insane and his grandmother to the highest degree eccentric.

I decided that his studies should be interrupted and that he should be placed in an institution where water treatment could be given. Here the hygienic program included gymnastic exercises, baths, and manual labor. These measures were intelligently employed under the direction of a wise and skillful physician, Dr. Gillebert d'Hercourt, and the physical condition of the youngster improved markedly. He grew appreciably taller; but another disquieting change came on and dominated the clinical picture. The young patient showed increasing forgetfulness of what he had previously learned; his intellectual faculties, formerly so brilliant, came to an alarming standstill. A sort of torpor or hebetude replaced his customary alertness, and when I next saw him I judged that the fatal transition *à l'état de démence précoce* was taking place. Such a hopeless outlook is ordinarily difficult for parents to accept and even for the physicians who are looking after these children.

Such nevertheless is the unhappy consequence of hereditary insanity. An abrupt arrest of all the faculties, *une démence précoce,* bears witness that the youthful patient has reached the limit of intellectual development of which he is capable.

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FIXED IDEAS

Des idées qui émeuvent et que, de ce fait, on dit vraies sont marqueés au coin d'un bien curieux caractère; elles semblent eĉhapper au temps, avoir été toujours là, proveuir de quelque fonds original de l'âme, d'ou s'élève l'esprit éphémère de l'être individuel comme une plante qui va porter des fleurs, donner des fruits et des graines, puis se faner et mourir. Les idées proviennent de quelque chose de plus grand qui l'homme personnel: ce n'est pas nous qui les faisons, ce sont elles qui nous font.

—C. G. Jung

# COMMENT

## PRELIMINARY REPORTS OF NEW OBSERVATIONS

This JOURNAL offers facilities in the section devoted to Correspondence, Clinical Notes, etc. for the publication of short reports of new observations which may have sufficient value to make early publicity desirable. Examples of such reports might be the benefits or the hazards of new treatment methods, modifications of standard therapies which have proved advantageous, new examination techniques, etc.

Lengthy articles running to a number of printed pages must as a rule take their turn in publication, and because of the backlog of manuscripts constantly on hand a delay of several months is inevitable. A preliminary report such as referred to here must not exceed 500 words. An accepted report reaching the editorial office not later than the 25th of any month can then appear in the second succeeding issue of this JOURNAL in the section for miscellaneous items. No tables or illustrative material may be included. The manuscript must be the original copy, double-spaced, and carefully edited by the author, without footnotes or bibliography.

And *nota bene:* 500 words, no more.

## SIN, CRIME, AND SICKNESS

It is not uncommon to hear some benevolent gentleman say, "I hate sin, but not the sinner." He may well seem to imply that he loves the sinner. In similar vein other benevolent persons—perhaps sociologists or psychiatrists—might say, we hate crime but not the criminal. It follows that we must fight crime but not *punish* the criminal. How do such statements square with semantics? "Sin" and "crime" are talked about as if they were commodities and there were certain unpleasant persons who deal in them. The logician however will admonish that sin and crime are abstractions and that talk about hating sin or crime apart from the respective doers is sheer nonsense. Disembodied sin and crime do not exist; sinners and criminals do. Nothing can be done about the abstractions except to talk about them; something can be done about the human beings who represent these abstractions and who alone give them any meaning. And that in fact is all, and exactly what, we attempt, however benevolent we may be, or how by the careless language we use we may do violence to logic.

The word sin as ordinarily used is a theological term with which psychiatry would not seem to be concerned. Unhappily however our race is still burdened with sin concepts, even with the absurdity of "original sin"; and there are specialists who grade and classify sins. And so it happens that many psychiatric patients express sin delusions, often of the most painful character. We cannot deal with the abstractions, but only with the patients and every practitioner knows how difficult the problem may be. For the patient sins mean punishment, even to indefinite sentence to the regions so authoritatively described in Part I of the *Divine Comedy.* And for the patient, the physician's assurance to the contrary, constitutes perjury. Since the delusions of mentally disturbed persons are apt to reflect, perhaps in distorted form, ideas prevalent in their cultural environment, it cannot be expected so long as concepts of sin are enmeshed in that culture that melancholy patients will be spared these most painful of all delusions.

In loose language there may be some relationships between sin and crime, but the differences are more striking. Sin, generally speaking, is subjective, may have little or no basis in fact and create no social damage. It is a state of mind that the sinner conceives deserves punishment which he may inflict upon himself and which he also expects *post mortem.* Crime is objective and creates obvious social damage. The criminal usually

seeks by every possible means to escape punishment.

Neither society nor the agencies of law and order or of health is concerned with "sin" or "crime" in the abstract but only with the persons to whom those terms may apply.

What have all these banal observations to do with sickness? In the first place the "sinner" may be merely a sick man and a psychiatric case. Not infrequently the criminal may be too, although it is not necessary to follow, as Guttmacher and Weihofen put it, "some radical theorists who go so far as to say that all criminals are mentally disordered." But the focus of interest in this present discussion is the view that sickness, like sin and crime, has no existence by itself, apart from the person who is sick. Consequently good medicine nowadays aims at the individual, whose nature, moreover, may help to explain the malady that afflicts him.

The ancient concept of disease entities taken over from medicine based on structural pathology and applied to psychological disorders in a great portion of which no structural patholgy has yet been discovered, has led to much confusion in psychiatry. First, Greek names were given to an assortment of morbid mental states according to salient symptoms or hypothetical causal factors that could never be demonstrated. Then began the business of classifying these pathological "entities" although no scientific basis for classification existed and still does not exist. Lacking such basis the classification had to be revised from time to time or a new one set up to satisfy changing views; but the hope of eventually establishing a definitive classification was hardly to be entertained. Then too the "entities" had no borders or even identities. A given case might fall into one category or another according to one or another diagnostician. One clinical type might seem to pass into another; an earlier diagnosis was said to be in error. Mixed types appeared. Cases that were considered hopeless surprisingly recovered; those that were expected to recover didn't. It was all very disturbing. Wounded diagnostic pride might have to give way to a humbler agnostic attitude.

It was realized, of course, that just as the skeletal joints are limited to a few definite movements, so the grosser trends of the thought processes follow a few recognized patterns, and likewise only a few terms are needed to name the several emotional reactions. It was these comparatively narrow limitations that gave rise to the ancient diagnostic labels that we still find convenient. But despite these limitations the human paradox was that the range of psychic experience, normal, abnormal, and pathologic, was virtually boundless. Whatever general symptomatic diagnosis might be made, an individual diagnosis had also to be made—and that was the important one. It was futile to continue forcing patients into Procrustean categories; each patient was a *unicum*. And so we lose sight of the "disease entity" and the abstraction of illness and come back to the person who is sick.

Sin, crime, and sickness we shall have with us for a very long time. It is not with these in the abstract but with sinners, criminals, and sick human beings that society is concerned. The self-accusing sinner is likely a person in need of psychiatric treatment; some criminals also have this need, but the question of penalties inevitably enters here too; and mental patients have at length come into their rights to be considered not only as social units but as individuals, with psychiatric diagnoses as such playing a somewhat diminished role.

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Dr. Benjamin Rush made this entry in his *Commonplace Book* under date of July 27, 1796:

"Mrs. Mease told me when dying that among other sins she had to repent of one was too much confidence in my remedies."

# NEWS AND NOTES

**Understanding American Life.**—Better understanding of American life is the major aim of grants totaling $427,325 announced by Charles Dollard, president of the Carnegie Corporation.

To strengthen its program in American studies, the University of Minnesota received $107,000 from the Corporation. Part of the fund will be used for a pilot study of science and technology as influential forces in American life; $25,000 was granted the Library of Congress for new recordings of authentic American folktales and folksongs. A grant of $90,000 went to the University of Nebraska for an education program for aiding the people of Nebraska to deal more effectively with their own community problems and to enrich their cultural and community life. The University of Wyoming was granted $40,000 for support of a program in international affairs.

Numerous smaller grants have been awarded to various other universities and colleges throughout the United States and British dominions and colonies.

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**Death of Dr. Taneyhill.**—The Maryland Association of Private Practicing Psychiatrists submits the following obituary note.

"On August 29, 1954, our esteemed colleague, G. Lane Taneyhill, died of a cerebral hemorrhage at the age of 74. We wish to record our sorrow at his death. He was a distinguished neurologist and psychoanalyst, who had performed fruitful services to both disciplines for long years. He gave unstintingly of himself during the war years when he was called from virtual retirement, forced by cardiac disease, to supervise the psychiatric end of the Maryland Selective Service System. Many of us will remember him as a kind man to younger colleagues, witty but never at another's expense. We shall miss his trenchant comments at our meetings. He is survived by an only daughter."

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**Menas S. Gregory Lecture.**—The annual Gregory Lecture was given by Dr. David McK. Rioch in the Amphitheater, Psychiatric Division, Bellevue Hospital, New York City, on October 21, 1954. Dr. Rioch is technical director of the neuropsychiatry division of the Army Medical Service Graduate School. The subject of the lecture was "Psychiatry as a Biological Science."

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**Thomas William Salmon Lectures.**—The twenty-second series of the Salmon Lectures was given by William Alvin Hunt, professor of psychology, Northwestern University, on Monday, Wednesday, and Thursday evenings, December 6, 7, and 9, 1954, at the New York Academy of Medicine. Dr. Hunt's general subject was "The Clinical Psychologist."

The subtitles of the 3 lectures were: "What a Clinical Psychologist is"; "How the Clinical Psychologist Came to Be"; and "The Problems and Future of Clinical Psychology."

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**Winter Seminar in General Semantics.**—The seventeenth Winter Intensive Seminar in General Semantics, consisting of 40 hours of lectures, demonstration, discussion, and group training, will be held from December 27, 1954, to January 2, 1955, at Lakeville, Connecticut, under the direction of Dr. O. R. Bontrager, professor of education, Pennsylvania State Teachers College, in association with the staff of the Institute of General Semantics. Dr. Bontrager has conducted the winter-holiday seminars for the Institute since 1950.

Enrollment is limited to 20 and admission requirements are flexible. Full and partial scholarships are available. Early registration is requested. For further information write the Institute of General Semantics, Lakeville, Conn.

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**Conference on Mental Retardation.**—The second annual conference on Problems in the Field of Mental Retardation was held October 29 and 30, 1954, at the Elwyn Training School, Elwyn, Pennsylvania, un-

der the auspices of the Pennsylvania State Department of Welfare and private and state schools for retarded children.

**The Indian Journal of Social Work.**—The March 1954 issue of this Journal is a special number containing a full report of the Sixth Annual Indian Conference of Social Work, reflecting the present status of social work in that country. The conference was divided into 4 major divisions: Social Work and the Five-Year Plan, Health education, Tribal Welfare, and Social Defence.

The section on health education considered mainly leprosy, tuberculosis, and venereal disease. The topic of mental health does not appear in the report. It is noteworthy also that none of the 60 or more delegates to the recent International Conference of Social Work in Toronto took part in the voluntary study group on mental health, and further that at the World Mental Health Conference in Toronto, only 1 or 2 delegates came from India.

The Indian Conference on Social Work was concerned with the political, economic, social, and educational fields, and the various aspects of public health and industrial development, indicating great activity in these fields. Apparently work in the mental health field still lags.

**Dr. Kirkpatric Heads Domestic Relations Bureau, New York City.**—On October 1, 1954, Dr. Harris B. Peck, director of the Bureau of Mental Health Services of the Domestic Relations Court of New York City resigned his position to devote himself to his practice and to research.

Dr. Price A. Kirkpatric who had been associated with Dr. Peck succeeded him as director of the Bureau. Dr. Kirkpatrick, a graduate in medicine of Northwestern University Medical School, has had experience in the Chicago and San Francisco Health Departments, the New York State Mental Health Commission, and is instructor in psychiatry, Cornell Medical School.

**Gerontological Research.**—Dr. Austin Smith, editor of the *Journal of the American Medical Association,* dedicated on October 2, 1954, the new Merrell Laboratory of Basic Medical Sciences in Cincinnati.

The new laboratory, set up by the Wm. S. Merrell Company, a pharmaceutical house now in its 126th year, will give special attention to research into the problems of aging and of the prevention and treatment of the disabilities of old age.

**Cleveland Society of Electroencephalography.**—At the first autumn meeting of the Society, held October 4, 1954, a symposium on the subject "Forensic Problems in Electroencephalography" was conducted. A committee of 4 (chairman, Dr. Andre Weil) to establish minimum standards of mechanical and professional competency in the making and interpreting of electroencephalograph records was appointed by Dr. Lawrence Weinberger, vice-president, who presided. The committee will draft a code of ethics which will be offered to the organization for ratification or amendment in the Spring of 1955.

## . . . TRAILING CLOUDS OF GLORY

To talk about the innocence of a child's mind is a part of that practical idealism and willing hypocrisy by which men ignore realities and delight to walk in vain shows; in so far as purity exists it testifies to the absence of mind; the impulses which actually move the child are the selfish impulses of passion. It were as warrantable to get enthusiastic about the purity and innocence of a dog's mind. "A boy," says Plato, "is the most vicious of all wild beasts"; or, as some one else has put it, "A boy is better unborn than untaught."

—Henry Maudsley,
*The Pathology of Mind* (1899)

# BOOK REVIEWS

**Understanding the Japanese Mind.** By *James Clark Moloney, M.D.* (New York: Philosophical Library, 1954. Price: $3.50.)

The central hypothesis of Dr. Moloney's book is that a country's psychological status may be revealed by its behavior toward psychoanalytic concepts. For the purposes of this book, the principal concept is that psychoanalysis aims to release the patient to act as an independent, self-directing individual, acquiescing to moral rules or rejecting them according to his own ethical or adjustment pattern. Moloney shows that Japanese society is based on ideas that are fundamentally opposed to this doctrine. He supports this with material from philosophical, religious, and historical sources. These forces of nonindividualization are demonstrated to have been too powerful for analytic concepts to conquer. Psychoanalysis has been blunted and changed in the hands of its Japanese interpreters, "syncretized," so that it has lost some of its fundamental and basic significance. This conclusion is supported by quotations from the Japanese literature on psychoanalysis, much of which Dr. Moloney has had translated. The conclusion is also documented by conversation with leaders of Japanese psychiatry and psychoanalysis, and by intensive correspondence.

The book is carefully documented and justly deserves to be called a scholarly piece of research. There is one unfortunate lapse in scientific procedure that must be pointed out. Dr. Moloney believes that schizophrenia should be less common in a culture in which individualism is "trained out" of people at a very early age. That this training is inculcated very early in the life of the Japanese child is clear from the material presented here, and elsewhere, about the duty to the father, the household, the emperor, and by the principle of "co-evality." This latter idea is a belief that Japan, its people, and its emporer, have always been, are now, and ever shall be, not a group of individuals but rather a unit. The doctrine is something like the ecclesiastical dogma of many Christian churches as regards the Triune God, though, of course, the continuation of the same persons is not implied in the Japanese concept.

There is an attempt by the use of statistics to support the thesis that such doctrine tends to protect against schizophrenia and to increase susceptibility to aggressive reactions turned inward. The low rate of mental hospitalization for Japan is adduced as evidence concerning the incidence of schizophrenia. The author suggests that this is not too good an argument because the Japanese insane are "tractable" and therefore managed at home, but nothing is said of the many other factors that vitiate the possibility of the use of such statistics. No differential diagnostic studies are given. To compare raw rates of hospitalization in Japan with those of Western nations, without specific consideration of economic situation, the paternalistic system, and ancestor worship, and the extremely strong family ties of the Japanese, is unwise. As a matter of fact, the one population survey on a group of 20,000 persons indicates rates higher for most mental diseases than is the case in the U.S., though the evaluation of the study is difficult. Similarly, it has thus far proved impossible to support the thesis that hypertension is significantly more common in groups known to show chronic suppressed resentment in the U.S., and it is doubtful that the frequency of apoplexy in Japan can be used as an argument to support the same thesis as regards the Japanese in whose country reporting practices are inferior to our own. It should also be pointed out that the Japanese patient is cared for by a far higher concentration of physicians, nurses, and other personnel than is his U.S. counterpart. It is my impression that this fact accounts to a considerable extent for the "tractability" observed. Italian patients are cared for similarly and they, too, are remarkably quiet as compared with American hospitalized patients.

Such criticisms are important to the central thesis of the book, but they do not deter from its essential value as a highly interesting and thorough and well-documented study of the history and the interpretation of the history of psychoanalysis in Japan.

A Japanese physician remarked, after reading the book, that many of the observations are accurate but that it should never be forgotten that, particularly since the war, Japan is a country in transition. Many of the older concepts of family solidity and duty are changing and are being replaced by greater freedom of action. He also warns that the cultural patterns observed by outsiders do not necessarily imply that the powerful emotional forces that they channel are not expressed privately and behind the cultural facade, though he admits that the culture does, for example, inhibit to some extent the expression of romantic love between husband and wife, even in the privacy of their own rooms.

Reading this work will result in a better understanding of Japan and the Japanese, though it probably will not achieve the result implied in the ambitious title.

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**Patterns of Organization in the Central Nervous System.** Edited by *Philip Bard.* (Baltimore: Williams and Wilkins, 1952.)

This book is a collection of papers given at the meeting of The Association for Research in Nervous and Mental Diseases held in New York on December 15 and 16, 1950. The papers are specialized in the field of neurophysiology, based on electrographic and oscillographic methods and represent the advances made by these techniques.

There are 4 sections to this volume, each with a summary. The first includes studies by Weiss on coordination in amphibians resulting from experimental interference in their premetamorphic stage of development. In this section also are papers by Kuffler and his co-workers on spinal reflex mechanisms, discussing the role of the small nerve fibers which facilitate afferent discharges from muscle spindles. Lloyd's studies on the role of synergists and antagonists in spinal reflexes and Bernard's studies on central reflex time are of great interest. In another paper, McCulloch and his co-workers present data to explain facilitation and inhibition of a monosynaptic spinal reflex in terms of electropotentials. A paper by Hines and Knowlton deals with a phenomena of action potentials observed in a contralateral limb reputedly following a blow to the patellar tendon in the supposedly experimentally deafferented opposite limb. In a final paper in this first section Magladery and his co-workers present studies on low threshold monosynaptic and high threshold polysynaptic reflexes in man.

The second section deals with facilitatory and inhibitory action of brain stem centers upon spinal reflexes. In the first paper of this series Brookhart arrives at conclusions similar to Kuffler in a previous paper, namely, that the small, slowly conducting nerve fibers, with their low stimulating potency, are responsible for the tonic elements of pyramidal function, while the large, rapidly conducting pyramidal fibers have a high stimulating potency and are responsible for the phasic elements of pyramidal control.

The following paper by Lindsley concerns the role of extrapyramidal influence on the pyramidal system and evidence is presented to show that the reticulo-spinal and vestibulo-spinal tracts are facilitatory pathways, while the cortico-bulbo, reticular, caudate spinal, and the cerebello-bulbo reticular tracts serve as suppressor pathways.

In the next paper Austin deals with depression and facilitation of spinal reflexes by stimulation of suprabulbar structures. The 2 last papers—one by Ward and the other by Woosley and his co-workers—give a pattern of localization in the cerebral motor area which is more detailed than the pattern described by Brodman. In addition to the precentral motor area, a supplementary motor area in the sulcus cinguli, rostral and ventral to the precentral motor area is described.

The third section deals with function of the cerebellum and its relation to the brain. The first paper by Snider gives a summary of the works on interrelation of cerebellum and brain stem appearing during the last 20 years. The author was able to find the characteristic fast cerebellar activity (300 per second) in different formations of the brain stem, such as in the inferior olive, lateral vestibular nuclei, reticular formation, and nucleus latero-ventralis of the thalamus.

Similar brain stem cerebellar activity (convulsive pattern) was found by Markham and his co-workers after injection of strychnine intravenously or into the cerebellum. They also found that mutual antagonism exists between the activity of the cerebellum and brain stem on one side and cerebellum on the other.

In the last 2 papers of this section localization of motor centers in the cerebellum are described and cerebello cerebral projections discussed.

The fourth section deals with anatomical distribution of receptive centers, the anatomy and function of the reticulo-thalamo-cortical system and the relation of neuroanatomical and physiological with psychological data.

The first paper by Mountcastle and co-workers is concerned with the potentials seen in the cerebellar and cerebral cortex after stimulation of various afferent nerve fibers conducting deep sensibility. Amassian discusses interaction in the somatico-visceral projection system and shows that inhibition and occlusion occur in the thalamus and in the cortex. He advances an interesting theory, namely, that poor perception of visceral changes may in part be due to subcortical block rather than failure of the fiber system to reach the cerebral cortex and that visceral awareness occurring in certain neuroses may be produced by modification of this subcortical inhibition.

Rush and co-workers studied the projection of somato sensory stimuli upon the posteroventral thalamus and also the projection of chorda tympani (gustatory fibers) and splanchnic nerve stimulation upon the sensory cerebral cortex.

The paper by Hsiang-Tung Chang deserves particular notice as it represents a revolutionary change in the present working hypothesis in explanation of color vision which is based on the assumption of threefold receptors in the retina. In contradiction to this theory Chang assumes the existence of 3 independent conduction pathways responsible for the conduction of the 3 fundamental colors.

Rose reports on studies of the thalamic reticular complex, a fiber system situated between the external medullary lamina and the internal capsule. The studies are based on retrograde degeneration after ablation of the neocortex. It has been concluded from these experiments that the thalamic reticular formation is connected with a larger portion of the neocortex than any single dorsal thalamic group of nuclei.

Magoun reports on the function of the reticular system in the brain stem. Stimulation of this system awakens a sleeping subject and diminution of afferent impulses predisposes the animal to sleep. Stimulation also produces electroencephalograms in the cortex resembling those observed in natural awakening from sleep. Destruction of its rostral portion leads to somnolence of the animal.

Jasper and Marsan in their paper on thalamocortical integrating mechanism discriminate two projection systems from the thalamus to the cortex, each of which plays a different role in the integrative system of the brain. The first is able to regulate afferent impulses to the sensory area of the cortex, the second, originating in the diencephalic reticular formation, controls the alpha rhythm of the cortex and it is assumed that the latter deals with alertness and consciousness.

The paper by Penfield discusses the interrelation-

ship between the center-encephalic system and the epileptic automatism. The center-encephalic system is defined as the structure within the brain stem which may be responsible for the integration of function of the 2 cerebral hemispheres, whereas brain stem, differently from definitions given in the textbooks, is assumed as being "all of the brain except the cerebellum, cerebral cortex and their dependencies"; it includes also the thalamus. Penfield proposes the existence of 2 mechanisms functioning within the center-encephalic system: mechanism A, used to record a man's current perceptions; and mechanism B, used in integration of motor and sensory systems, the acquired skills of speech, manual dexterity, and recollection of past experience. Local discharge in the center-encephalic system is capable of producing petit mal attacks while disturbances originating in the frontal or temporal lobes result in automatic or amnesic states. Seizures starting in the sensory or motor cortex result in sudden generalized convulsions always with unconsciousness.

The last paper presented by Lashley deals with integration in general. He demonstrates that patterns of behavior differ completely from those of structure in the CNS. Anatomical localization gives no explanation for the higher function of integration. In spite of the knowledge of the different fiber tracts and of the cerebral localization the link between neurophysiology and behavior is still missing.

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VERSE-DIARY OF A PSYCHIATRIST. By *Merrill Moore.* (Baltimore: Contemporary Poetry, 1954. Price: $3.00.)

Having no competence as literary critic, the reviewer can only say that he found delight—although that is hardly the expressive word—in the pictures called up by the 33 poems by America's phenomenal sonneteer and contained in this slender and distinguished volume. Merrill Moore has the gift of taking incidents and situations in the daily round, embuing them with poetic significance, and making the reader experience them with him, and remember them too. And his art is in making these word pictures seem artless, almost casual, yet fully convincing. They will stand rereading, and become still more alive in the process. You say, that fellow is just talking to you, saying ordinary things to make them unordinary, in that special speech of his which is poetry.

Number one in this book, *In No Time at all I was where I Meant to be,* records the attainments and rewards of early life, seemingly at cost of little effort.

And why should that be so?

"Because I always felt
the bars between two people ought to melt
In the heat of friendship and they usually did."

Those lines make one think of Robert Frost's

"Before I built a wall I'd ask to know
What I was walling in or walling out."

And in other lines too in these sonnets one seems to catch a Frostian echo, and they are none the worse for that.

There are vivid glimpses into human minds in these poems, sick minds and everyday minds in their peculiarities and vagaries. The Walter Mittys would improve their technique by learning that

"To accept one's normal limitations
Enables one to perform more effectively
Within them."

In the sonnet about "The Suffering Ones," what deeper insight or better lesson in psychotherapy than

"They wanted to be spoken to in their own
language,
They want to hear the syllables they know
Uttered in a tone they understand."

A Pavlovian note is struck in the poignancy of sad memories in the poem, *Once a Man, Thinking of Forsythia, Wept.* Here it is:

"I wondered what the tricks his memory
Might have in store to let aloose on him
Thus bringing tears.
I knew that as a child
His life had been hard, that they had been poor;
Forsythia was the only plant that bloomed
Once, he told me, by their kitchen door.

They had no time for flowers; father died
With tuberculosis, Mother also was sick.
All the children did their share of work
And work for neighbors cut him to the quick.

So I think that when he saw forsythia
Blossoming in gold upon its bush
Much of his unhappy past would rise
Up in his mind and strike him with a rush."

C. B. F.

SAVING CHILDREN FROM DELINQUENCY. By *D. H. Stott, Ph.D.* (New York: Philosophical Library, 1953. Price: $4.75.)

Dr. D. H. Stott, Ph. D., is a Research Fellow of the Institute of Education, University of Bristol, England.

The material was collected while the author was preparing a report on delinquency under the auspices of the Carnegie United Kingdom Trust. For the purpose of the present discussion, delinquency is defined as being actual law breaking which has been detected by law enforcement agencies, in contrast to the fairly common practice of defining delinquent behavior in terms of the moral views of the observer.

This practical approach is the basis for the arguments justifying treatment and prevention in terms of economics. Fundamentally, the thesis presented by Dr. Stott is that delinquency occurs when a child is faced with an emotional situation he cannot handle. It is not the sole mechanism for solving the problem, others being the various types of neuroses or psychoses.

The principal reason for the selection of one of

these in preference to others, in a given case, is considered to be the family mores. Lax parental morals contribute directly to delinquency, while the pressures of over-rigid, puritanical regulation more usually tend to produce neurotic or psychotic behavior. Less frequently it happens that the family standards of achievement are set so high that the child's slightest failure to live up to them produces intense parental rejection and in these instances either delinquency or neurotic behavior may occur.

Prevention of delinquency is considered to be a matter of finding the families in which the emotional climate is sufficiently unhealthy to produce delinquency, and modifying or eliminating the conditions producing the unfavorable reaction. In spite of the poor housing and economic conditions, which constitute the common denominator in the majority of cases quoted, it is felt desirable to leave the child in the home if there exists any possibility of changing the faulty parental attitudes.

Looking back on the case histories of 102 boys in an "approved school," Dr. Stott feels that in 64 of them delinquency probably could have been prevented by proper handling in the home. Admitting that this is an inadequate sampling statistically, he uses it merely as an indication of what might reasonably be expected with more effective guidance and counseling methods.

Unquestionably our knowledge of the family structure and its emotional implications is inadequate and poorly organized. Signs of emotional stress frequently go unrecognized in the early stages when treatment is more hopeful, and, more often than not, no steps are taken until the situation has deteriorated so seriously that radical measures are necessary, and the outlook necessarily less optimistic. Dr. Stott stresses the importance of a more realistic study of family relationships, and, as a practical suggestion, has given a diagnostic outline of the factors predisposing to delinquency which might be observed by such authorities as probation officers. Of much more general interest, however, are his suggestions for the diffusion of knowledge about mental health, and his specific recommendations that young married couples should be given some sort of special training to prepare them to handle the emotional implications of family responsibilities. This program is recognized as an ideal that will take time to bring to realization.

In an appendix, the relationship between mental dullness and delinquency is discussed, and it is pointed out no simple causal connection exists, in the sense that the delinquent is necessarily dull or the reverse. On the other hand, the emotional disturbance that produces delinquency as one symptom may simultaneously produce apparent intellectual retardation as another. Treating the underlying emotional problem, therefore, may produce an improvement not only in behavior but also in the intellectual level, as determined by standard tests.

In the introduction, Dr. Stott describes his work as an essay "to dispel any anticipation of this being a comprehensive treatise." The latitude implied in the choice of this literary form allows the writer considerable freedom, and it is open to question whether all the inferences made in the special situation described would be valid in other circumstances. One difficulty in making the comparison is created by the assumption throughout the text that the reader is thoroughly familiar with the specifications and functions of the English "approved school" and Borstal system, which probably is not the case with everyone on this side of the Atlantic.

One has the feeling after reading this book that the author has fulfilled his promise of writing an essay, and one might add that he has done an excellent job in producing a stimulating philosophical discussion in the best literary tradition. The possibilities of the procedures of investigation, prevention, and treatment suggested by Dr. Stott are worthy of further thought and discussion.

LESLIE R. ANGUS, M. D.,
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DIE PSYCHIATRIE DER HIRNGESCHWUELSTE UND DIE CENTRALEN GRUNDLAGEN PSYCHISCHER VORGAENGE. (THE PSYCHIATRY OF BRAIN TUMORS AND THE CEREBRAL BASES OF MENTAL HAPPENINGS.) By *Dr. Hans Walther-Büel.* (Vienna: Springer, 1951.)

This is an interesting study published from the material of the psychiatric clinic of the university of Zurich. It has a preface written by H. Krayenbuhl, professor of neurosurgery at Zurich and another by M. Bleuler, professor for psychiatry, Zurich.

Walther-Büel found in 600 brain tumor cases, 70% mental disturbances, the majority of which were of the subacute or chronic exogenous reaction type. He found in 38% of the cases a dimmed consciousness; in another 38% the so-called "psychosyndrome" of E. Bleuler; in 12% a symptomatic epilepsy with mental disturbances. It seems to be characteristic for brain tumors that they have a combination of all the syndromes. The organic nature of mental disturbance shows itself for a long time only in the form of not very characteristic personality changes. We often find a picture of presomnolence, a syndrome described very well by Stertz. The organic process plays the principal role in the pathogenesis of mental disturbances. The constitution of the personality is not too important, but it can often color the organic picture. There is a minority of 4.5% cases in which a specific hereditary and constitutional disposition is of great importance. In those cases the organic process loosens the latent trend to a psychosis. Age plays a great role in pathogenesis in regard to the resistance of the brain. Chronic brain pressure causes in many young brains the reversible syndrome of a dimmed consciousness with a trend toward coma. In the aging brain (beginning with age 35 or 40) it makes a more lasting organic "psychosyndrome" ending in dementia. The majority of mental disturbances in brain tumors are the expression of diffuse organic brain damage. Only 5.5% of the whole material like narcolepsy, like frontal lobe syndromes, like the "Uncinatus Auras," are of local diagnostic significance.

The book ends with an interesting chapter on localization of mental processes. Here the author gives a historical review. He himself thinks that the cause of mental happenings can never be explained by happenings in a certain part of the brain. It is always the brain as an unit. He thinks that brain surgery has helped much in the knowledge of localization even if in a negative way.

WILLIAM MAYER, M. D.,
New York City.

THE STUDY OF BEHAVIOR. Q TECHNIQUE AND ITS METHODOLOGY. By *William Stephenson.* (Chicago: University of Chicago Press, 1953.)

"*The Study of Behavior* offers a striking *méthode rationale* for the direct operational study of self-psychology and psychoanalysis. It shows us how to experiment with theories that have been erroneously examined and never shown either credible or false. It brings the method of factor analysis and variance design, the small sample theory, into the laboratory and clinic for experiments in the single case. . . . In all these applications [of the Q method], it is shown that studies can be pursued for the single case without norms, scales, or any dependence upon individual differences as postulatory matters. Theory is given the central position; and we are told what facts we can best look for in the study of behavior."

It was the foregoing two paragraphs from the publishers blurb which particularly attracted me to Stephenson's book, and I am glad I had a chance to read and study it.

Ever since 1913, when I was engaged as a medical officer of the United States Public Health Service, in the mental examination of immigrants at the Port of New York, I have been hoping that someone would come along and put some sense into the protean array of "intelligence tests."

During the heyday of this new dispensation, following Goddard's translation of the Binet-Simon intelligence tests, it seemed as though almost every candidate for a Ph. D. degree produced a new scale of intelligence tests.

Having accepted for a long time now the principle of the "universality of uniqueness," first enunciated, I believe, by the late H. G. Wells, I never could get myself to take these tests seriously, at any rate not until the arrival of the various projective methods upon the scene. I think the Q method, which this book deals with, furnishes the long-awaited stable and dependable frame of reference for dealing with this entire subject, even though I readily acknowledge that some parts of this book are beyond my depth.

In this prolegomina giving his reasons for the development of the Q technique, Stephenson has the following to say.

"It was our purpose to provide 'Q' with its own set of definitions and postulates, for reasons which we could grasp at once in terms of much that had been discussed in psychology for nearly a hundred years and which never had a satisfactory answer up to then. We considered it important to rid at least one factor technique from, on the one hand, any postulatory dependency upon individual differences, and on the other, from protopostulations of a seriously limiting kind, upon which R was based. We had, therefore, to construct a methodology for the single case: we call it 'Q Methodology'."

The reasons for this are not far to seek in terms of a background that a Spearman student had to have in the latter part of the 1930's. For we were oriented toward the history of psychology and we knew our Ward, the cross-currents that were critical of the "elementalism" that conceived of man as a mass of "characterists," such as, his height, temperament, intelligence, and the like, which could be studied in terms of individual differences. Spearman himself could not grasp the force of these criticisms. He regarded Gestalt psychology, for example, as a sort of "public enemy No. 1."

Besides, he was fascinated by the elegancies of R technique which offered to deal sensibly with man's "characteristics" in a way hitherto quite impossible of achievement. But at least one of his students strayed: Q technique, we were sure, could represent the newer, but really old, viewpoint of a Dilthey or a Ward. It would do this if it could deal with "wholes," with "descriptions," with the "concrete" person.

Psychiatrists, indeed all clinical workers, will readily go along with Stephenson as regards his avowedly "holistic" point of departure.

As regards the actual nature of the Q technique as an instrument for the understanding of the total personality, I can do no better than quote Stephenson's precise and clear statement of the matter by way of a single example he furnishes. "We begin with an example of a simple kind which has reference to the Szondi test. This, as is well known, consists of forty-eight photographs, each on a card showing the face of a former mental patient, eight each for former sadists, hysterics, catatonic schizophrenics, paranoids, depressives and manic patients. The test itself is a projective instrument and not the kind that R technique was ordinarily designed to study. If we regard the forty-eight cards as an example for Q technique purposes, they can be thoroughly shuffled (as for a pack of cards in bridge) and then quantified in many different ways. Thus we might ask a choir boy who has, of course, no idea what the cards really represent, to look at them all and then to score them for (A) which he likes best. To help him along we would provide a frequency distribution for him to work to. Thus he gives eight marks to the two photos he likes most and zero to the two he likes least, and so on, with most of the cards gaining three, four and five marks in the center of the distribution. The act of so scoring a sample of cards is called a 'Q test' for short.

"But we might also invite our choir boy, next day perhaps, to score the same cards for (B) which he believes to be the most Godfearing. Or (C) which is the most handsome. Or (D) which is the healthiest. Or (E) which is the oldest, and so forth. In each case the same frequency distribution may be used, the cards themselves, of course, usually gaining quite different scores under these varying

conditions of instruction. The arrays of such scores for A, B, C, . . . can be correlated (N equals 48) and factored, all with respect to the same choir boy. This, in miniature, is the kind of data dealt with in Q technique and it seems straightforward enough. But there should be a theoretical reason for the study, which will determine what the particular conditions of instruction will be and why this particular choir boy is the subject of experiment. If he happens to be suspect for schizoid condition, for example, we would be wanting to see not what cards he selects as such in relation to norms, but what relation, if any, exists between the various Q sorts he makes under the various conditions of instruction. That is, the interest would be to probe in a more detailed and individual manner into the particular case. Moreover, we would do the probing in terms of Szondi's theoretical framework all the time. Indeed, along such lines it is possible to put Szondi's theory to test, by way of detailed studies of single cases, using the factor analysis (and as we shall see, variance design as well) as a statistic for testing propositions about the case in question. Such in a very brief outline is Q. Technique."

It is clear from the foregoing that the Q Technique, by using a sampling of the same data in accordance with an unlimited number of instructions as regards the questions one would wish to reach answers to, renders possible the assessment of the same individual from all sorts of facets, independent of the basically unimportant question of individual differences.

I think we might justly assume that all human beings are basically alike, domesticated higher mammals, as someone has designated the human species. But it is undoubtedly true that no two human beings are exactly alike; this is perhaps true also of identical twins. But the differences between any two human beings, no matter how slight these may be, is of very great importance for the destiny of a human life. A technique, therefore, which offers an unlimited array, for all practical purposes, of approaches to the understanding of a specific individual human being should be highly welcome to the clinician, both as investigator and therapist.

BERNARD GLUECK, M. D.,
New York City.

THEORETICAL ANTHROPOLOGY. By *David Bidney*. (New York: Columbia University Press, 1953. Price: $8.50.)

This work is a study of postulates and assumptions underlying the development of cultural anthropology. It draws attention to the fact that anthropology, like other sciences, has its body of theory, as well as its capital of empirically established facts. The results, which deal with the concepts culture, myth, human nature, cultural dynamics, value, freedom, etc., are impressive and constitute a major contribution to the social sciences and social philosophy.

Two sections will be of particular interest to psychiatrists. The first, on myth, which Bidney defines as belief left behind by the advance of thought, has application in the study of myth-making and deviant behavior. The author limits his analysis to belief content and ignores form and function of form.

The second, an analysis of primitive thought, rejects Malinowski's statement that "Primitive man had his science as well as his religion" and substitutes "prescientific" for Levy-Bruhl's unfortunate term "prelogical."

EDMUND S. CARPENTER, PH. D.,
University of Toronto.

HYPNOSIS IN MODERN MEDICINE. Edited by *Jerome M. Schneck, M.D.* (Springfield: C. C. Thomas, 1953. Price: $7.50.)

This book, a symposium edited by a psychiatrist, contains 11 chapters, each by a different author, all of whom are well experienced both in their field and in the utilization of hypnosis in their specialty.

The separate contributions cover the history of medical hypnosis and its application in internal medicine, surgery, anesthesiology, dermatology, obstetrics and gynecology, psychiatry, child psychiatry, dentistry, and physiology. A final chapter concerns instruction in hypnosis.

Each chapter is ably presented in a simple, matter-of-fact fashion. Ideas and findings based upon actual and repeated experience, not wild claims or wishful thinking, characterize the entire book. The only weak chapter is the one on instruction and, in simple fairness to the author, it must be recognized that only general instructions can be given. However, he most ably emphasizes the fact that a good hypnotic technique depends upon adaptability to the individual subject and his problem and not upon routine procedures.

As might be expected in a symposium, there is some overlap and some variation in the presentation of ideas. Rather than being a defect in the book, this is definitely an asset, since anybody desiring a comprehensive idea of hypnosis should have the separate views of more than a single author.

Each chapter has a good bibliography and the book is well indexed. It is a noteworthy contribution to medical literature on a most difficult subject.

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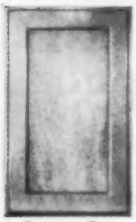

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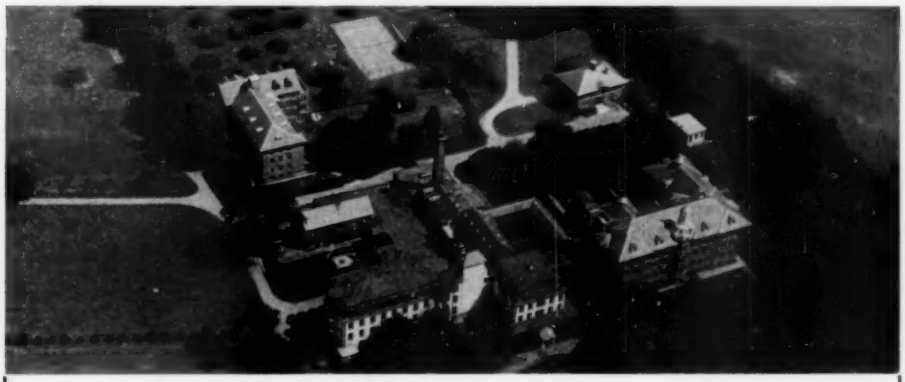

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